

JOURNAL
of the
Society for Psychical Research
VOLUME 40 No. 704 JUNE 1960

THE PALM SUNDAY CASE: A NOTE ON
INTERPRETING AUTOMATIC WRITINGS

BY W. H. SALTER

WHEN in 1906 I was first confronted with the problems of automatic writing through the paper (*Proc.* 20) in which Mrs Verrall discussed the scripts written by her in the first few years of her automatism I sensed that there was a meaning in them, or in some of them, without being able to grasp what their general purport was, or why it should have found expression in a style so alien to her habitual lucidity of thought and utterance. It is not surprising that when the situation became complicated through the production by several other automatists of an enormous mass of scripts, amounting with those of Mrs Verrall to over three thousand, and showing various signs of interconnection, the attempts of the very able and patient interpreters, particularly Alice Johnson, J. G. Piddington, G. W. Balfour, to explain them and to illustrate their explanation by selected quotations, should have left many of their readers bewildered.

The scripts deal with a great number and variety of subjects, some being events concerning particular persons and others such topics as the process of communication, and from an early stage anyone attempting to explain any one subject had to overcome the difficulty that there was in the scripts such a complicated interlocking of subjects that a fair presentation of one involved some elucidation of several others. This is well shown in Piddington's long paper in *Proceedings* 22 (see in particular the diagram and tables facing p. 280), and developments between 1908, when that paper was first published, and 1929, when the last of the scripts quoted by Lady Balfour was written, did not simplify the problem. Her observations (*Proc.* Part 189, pp. 204-8) and those of

Piddington (*Proc.* 33, pp. 443-60) to which she refers explain the methods by which the interpreters were able to arrive at an understanding of the general scheme of the scripts through observation of the way in which scripts were linked together and the methods by which references to persons and to topics were made in this very large mass of material. It may perhaps be of interest if I offer some comments on the general scheme of which the Palm Sunday case is a very important part.

The interpreters came to the conclusion that there was a pattern discernible throughout all the scripts, though in places it was blurred and imperfect: that this pattern was set out in an intentionally cryptic manner, but with increasing clarity, especially from 1912 onward: that the central part of the pattern was to declare that there was, and had for a long time been, in operation a plan promoted by a large number of intelligences who had survived bodily death to inaugurate a new world order based on international peace and social justice: that, among this much larger number, was a group of seven 'communicators' specified either by name or by appropriate symbols: and that connected with this central design were other designs, also cryptically expressed, intended to identify the communicating group, to prove their survival, and to illustrate the parts that the different members of the group played in advancing the plan. That was their interpretation of the pattern, which did not necessarily involve their acceptance of the plan as a reality.

The seven communicators are Henry Sidgwick, Frederic Myers, Edmund Gurney, Francis Maitland Balfour, Annie Marshall (née Hill), Laura Lyttleton (née Tennant) and Mary Catherine Lyttleton. The first three were the principal founders of the S.P.R. All were known personally to Mrs Verrall, and the whole body of scripts produced over thirty years had its origin in her wish to give Myers, if he had survived death, an opportunity for communication. It would therefore have been pointless to refer to him, or the other two founders, cryptically, and in fact they are referred to without disguise by their names or initials in Mrs Verrall's scripts and in those of the other three automatists concerned in the Palm Sunday case.

None of the other four communicators had during their life shown interest in psychical research, and there was no obvious reason to suppose that they would be associated in any psychical affair with the founders of the Society. F. M. Balfour (F. M. B.) was personally known to Mrs Verrall and is twice mentioned by name in her scripts. For the most part however both in her scripts and those of the other automatists he is referred to by the symbols

specified by Lady Balfour on page 103 of her paper. It will be noted that while all these symbols are appropriate, some are more distinctive than others. Franciscan and seraphic symbolism is appropriate to any person having the common name Francis. Deaths in Alpine accidents are regrettably frequent. Fishing is an absorbing interest to many. Scientific interest in the evolution of fishes is a good deal rarer. It is only by combining all these symbols that a particular individual can be precisely indicated.

The same, in a lesser degree, is true of Mary Catherine Lyttelton (M. L.), the principal communicator in the Palm Sunday case (see pp. 102, 103). Symbols naturally associated with common names like Mary (or May) and Catherine, lilies, mayflower (including the famous ship of that name) and wheel, would not separately, or even in conjunction, go far towards identification. But combine with any of these symbols allusions to the candle shewn in the photograph of her, and the scallop or cockle shells on the Lyttelton shield, and a much more distinctive element is introduced, while the references to the Hair, the ornamental box in which it was kept, the story of Berenice, all going back to early scripts of Mrs Verrall, seem to me to put the identification beyond all reasonable doubt. But only when viewed retrospectively after Mrs Willett's scripts of 1912, 1913, which are the main features of the Palm Sunday case.

These lay great emphasis on appropriate dates, especially of course the date of M. L.'s death, and throughout the scripts dates are often significant as allusions to particular events and persons. Sometimes the allusion is secondary, dependent on some more direct symbol. Thus the Feast of the Purification, mentioned in a script quoted by me on page 14 of Part 187 of *Proceedings*, is also known as Candlemas, and is therefore an appropriate if indirect reference to M. L.

There are also symbols appropriate to the persons referred to in the scripts, but appropriate not because of any verifiable event in their lives, but because of the part they play in the scriptic drama, *as interpreted*, and only appropriate if that interpretation is substantially right. There is no doubt, I think, that the 'Hand of the Master' means F. M. B., or that the Blessed Damozel and the Moon are used as types of M. L., but it is only through the scripts that the connection can be established.

The meeting of Dante and Beatrice in the Earthly Paradise is an instance as quoted in the scripts, of both kinds of symbolism. Apart from one detail it might be used to typify the case of any woman who has died young leaving a lover who cherished her memory. It becomes a specific allusion to A. J. B.'s devotion to the memory of M. L. through the incident of the emerald ring, the

emphasis laid on 'the emeralds' in Canto XXXI of the *Purgatorio*, and the mention in the scripts of the 'emeralds which must be found.'

Just as the conjunction of several symbols connected with one person may make a distinctive reference to that person, so the conjunction in a single script of the symbols of two or more persons may point to their connection in the plan cryptically set out in the whole body of scripts. I would once more refer to page 14 of my paper on Myers's posthumous message (*Proc.* Part 187). As there explained, there are in the H. V. script there quoted two allusions to F. M. B., 'Caller Herrings', and the paraphrase of Milton's lines, 'Where the bright Seraphim'. In relation to M. L. there are two dates of significance, the date of the script, 25 March, an anniversary of her burial, and 'the feast of the purification', also known as Candlemas. The crook and pastoral staff are less distinctive symbols, as being appropriate to any bishop. The close connection with Candlemas makes it probable that Bishop Talbot, in whose house the Hair was kept, is the bishop indicated. Other scripts refer to Keble College, Oxford, of which he had previously been Warden. None of these persons are named and my wife (H. V.) who wrote the script had no reason to connect them or any two of them.

The other two communicators, Annie Marshall ('Phyllis') and Laura Lyttelton, are also referred to by symbols, but as, after Mrs Verrall had read in January 1905 Myers's *Fragments of Inner Life*, she knew all the story of Phyllis, and as the story of Laura Lyttelton was well known to Mrs Holland, there was no point in maintaining any mystery about either of them. After the first few years they cease to play any important part in the script drama. M. L.'s identity, on the other hand, was concealed until disclosed by the scripts of Mrs Willett as set out in Part II of Lady Balfour's paper, as was also that of A. J. B.

History, including recent history, and literature provide numerous examples of the use of symbols to denote persons who, for political expediency or any other reason, are left unnamed. A good example is Act III Scene II of *Richard III* where, within seventy lines, Richard's personal badge, the boar, is seven times mentioned, once by Stanley's messenger and six times by Hastings, neither of whom mention Richard by name. But other reasons, besides political caution, are conceivable. One can only infer an intention on the part of the script-intelligence to keep for a time the automatists and the interpreters in ignorance of the inner meaning of the scripts from the fact that they were very effectively so kept. I would extend to the scripts as a whole what Lady Balfour says

(p. 172) about the cryptic presentation in early scripts of the Palm Sunday story, and (pp. 108, 109) as to the reason for the disclosure made through Mrs Willett's scripts, beginning with her script of Palm Sunday 1912.

In this paper I wish to confine myself to the question of interpretation and not to argue who, or what, the script-intelligence was, or were,¹ or whether there is such a plan for world order, as the scripts assert: still less, of course, to endorse the claim that the plan will succeed. But that the scripts do, on their true construction, assert the existence of such a plan and identify the seven members of the communicating group as amongst its promoters I have no doubt, and with a few reservations I accept the views of the interpreting group in this very complicated affair.

Concealment over many years of the design of the scripts from both the automatists and the interpreters was facilitated by the circumstances connected with Mrs Verrall's scripts and her attitude to them. The idea of a long-term plan being disclosed through automatic writing was not one that would naturally, in 1901, have occurred to her or anyone else. Having the reports of the Piper mediumship in mind, she naturally was on the look out for material that might be evidence of Myers's *post mortem* activity, or of her own paranormal faculties in relation to contemporary events or such as followed soon after apparent references to them in her scripts. The second condition favourable to concealment of the design was that her scripts, which began, as first attempts usually do, with scrawls and illegible words, continued for months to consist largely of an extraordinary polyglot jumble, Greek, Latin and English, Greek and Latin words, often of problematical meaning, being invented *ad hoc*. The whole was disconnected and ungrammatical. These oddities recur, but with diminishing frequency over several years: see the examples she gives in her paper (*Proc.* 20, 340-53).

Her early scripts may fairly be called nonsense scripts. Practically the whole of them look nonsense and much of them are, I think, really that. It would however be imprudent to pick out any word or phrase as having no meaning, because careful study shows that many apparently meaningless words and phrases are grouped in a way suggestive of a meaningful design: the 'evergreen image of dew', for instance, discussed by me in *Proceedings* Part 187. In this way the script-intelligence could unobtrusively stake a claim against the time when the identity of Phyllis, or M. L., or the other persons concerned in the script-drama came to be disclosed.

As an example of the way in which references to persons were

¹ In my concluding paragraph I comment briefly on this question.

built up, and not only in Mrs Verrall's scripts, from small and seemingly irrelevant beginnings, I would refer to the second part of the paper H. V. contributed to *Proceedings* 45 (pp. 39-41) in which she shows how a script of hers written in 1908 and containing the words 'three blind mice' develops in 1914, via the nursery rhyme 'A ship, a ship a-sailing' and Kipling's story *The Disturber of Traffic* in which that rhyme is quoted, into a reference to a lighthouse. This is itself a reference to M. L., as, although H. V. does not say it, the particular lighthouse which is the scene of the story is, as she knew, St Catherine's Lighthouse on the Isle of Wight. (Kipling calls it St Cecilia's but from his description he was clearly meaning St Catherine's.)

Some of the ways by which the scheme developed in the scripts without the automatists, or the interpreters, for a long time grasping it, have already been mentioned. There is the use of symbols, the uniformity of which not only in the scripts of the four automatists mentioned in this paper, but also in those of Mrs Stuart Wilson, who never knew anything of the story of A. J. B. and M. L., I find very impressive: the use, sometimes the indirect use, of significant dates, e.g. 'the feast of the purification'; the occurrence in an apparently meaningless context of meaningful words and phrases, of which more examples are given later; the extremely roundabout approach to a point of interest, as with the 'fairy-ship' and St Catherine's Lighthouse. To these may be added: slips, ostensibly unintentional but helping to establish a point, as (*Proc.* Part 187, p. 26) the mis-spelling 'Lith Hill', correctly spelt earlier in the same script 'Leith Hill', points to the maiden name of 'Phyllis' which was Hill: the ambiguous use of names, as (see p. 174 of Lady Balfour's paper) 'Arthur' and 'Wellington'. Especially in Mrs Holland's scripts, the surface meaning is not always the true one, or does not convey the true meaning in its entirety. Thus a script of hers written in 1909 which begins with a mention of the crest of the Fieldings and contains the phrase 'Eugene the Paladin', would appear to refer entirely to Everard Fielding's sittings with Eusapia Palladino, in which she was keenly interested, were it not that the only other occurrence of 'Eugene' in all her scripts is in a group of them written three years before and connecting the name with Alpine accidents. So the 1909 script is thus referring cryptically to F. M. B. as geneticist.

I decline to believe that these peculiarities, so numerous, so various in form but all tending to produce the same effect, are fortuitous. The script-intelligence must be credited with a skill and versatility in planting clues which would do credit to any author of who-dun-its, Mrs Willett's scripts being the equivalent

of the show-down in the last chapter of the story. As the devices mentioned occur and recur, in the scripts of Mrs Verrall, H. V., and Mrs Holland, it seems to me difficult to equate the script-intelligence with the subconscious mind of any one of them.

To revert to Mrs Verrall's 'nonsense scripts', first references to topics are introduced into them in the same way as the personal references. There are for example to be found there the first allusions to the foundation of Rome and to the Pax Romana as a prototype, admittedly an imperfect prototype, of the new world order. Myers had already used the foundation of Rome as a prototype, but with a difference. In his Presidential Address (*Proc.* 15, 126) adapting the famous line from the *Aeneid* (I. 33) *Tantae molis erat Romanam condere gentem* (Such work was it to found the Roman people—Mackail's translation) he speaks of 'the mighty struggle *humanam condere gentem*', and the last paragraph of *Human Personality* (omitting the Appendices) contains this passage, based on *Aeneid* II, 270-97:

And even as it was not in truth the great ghost of Hector only, but the whole nascent race of Rome, which bore from the Trojan altar the hallowing fire, so . . . the whole nascent race of man . . . forth from the Sanctuary of the Universe carries the ever-burning flame: *Aeternumque adytis effert penetralibus ignem* (he brings forth from the inner shrine the everlasting fire).

When she began automatic writing in 1901 Mrs Verrall must have known of Myers's adaptation of the line from *Aeneid* Bk. I in his Presidential Address of the previous year. She may have known that he had paraphrased the same line in a book of verse (*The Renewal of Youth*) published by him in 1882. The second passage she could not have seen as all the four scripts to which I am about to refer were written before the publication of *Human Personality* in 1903. It was doubtless her knowledge of Myers's devotion to Vergil, and of his adaptation in his Presidential Address of the Vergilian line quote, that stimulated her subconscious to make several quotations from the *Aeneid* in her early scripts. The remarkable thing is that, knowing her Vergil as well as she did, she only recognised the source of one of them: this can be shown both from the list of quotations given in an Appendix (pp. 357-360) to her paper in *Proceedings* 20, and the absence of any reference to any of them, without exception, in the manuscript books in which she entered contemporary copies of her scripts on one page, and, on the opposite, notes of such literary sources as she recognised.

Here are the script quotations in chronological order. The context, which is usually meaningless, or at least seems so, has been

omitted, except in one instance: the passage in the *Aeneid* from which the words are quoted is added in brackets. Mrs Verrall's first script was written on 5 March 1901, and it will be seen that the script-intelligence is quick off the mark

8 March 1901: *horresco referens* (*Aen.* II. 204—I shudder to recall it)

This was the second script she wrote.

9 March 1901: *quantum mutatum ab illo* (*Aen.* II. 274—how changed from him, i.e. Hector the great warrior)

This is the only quotation of this series listed in *Proc.* 20.

12 March 1901: *ineluctabile* (inescapable, *Aen.* II. 324 and VIII. 334)

This seems to be a rare word. Of three references to it, in the figurative sense, given in the dictionary, these are two.

14 March 1901: *carmentalis* (*Aen.* VIII. 338: see below)

The second book of the *Aeneid* tells Aeneas' story of the fall of Troy, and his escape from it, which led in due course to the foundation of Rome. *Horresco referens* is part of his account of the portent of the serpents which devoured Laocoon and his sons, and *quantum mutatus ab illo* is the description of how the ghost of Hector appeared when handing to him the sacred fire. In the eighth book Aeneas visits Evander, an Arcadian who had settled on the future site of Rome. *Ineluctabile fatum* (inescapable fate), says Evander, had driven him there, and the warnings of his mother, the nymph Carmentis. It was in her honour that later one of the gates of Rome was called Carmentalis. The fact that the word *ineluctabile* occurs in the two books of the *Aeneid*, and seldom anywhere else, might be thought to have no bearing on the scripts were it not that the next script to be mentioned also contains quotations from both books.

24 April 1901: *nox umida caelo* (*Aen.* II. 8—the dew of night from the sky) and *Carmentalelem* (sic) (*Aen.* VIII. 338)

The first quotation comes from Aeneas' preface to his story of the fall of Troy. In her MS notebook Mrs Verrall does not comment on either of these quotations, but she lists the first in *Proceedings* 20, p. 358. Before the word *carmentalelem* there come in this script the words *ambusca voce condidit opem*. *Ambusca* is not a Latin word. It is one of Mrs Verrall's odd inventions, obviously based on such English words as ambush, ambuscade. If *Carmentalis* may be considered as embodying the notion of prophecy, the phrase may be translated 'In cryptic language he has hidden a wealth of Roman prophecy'.

Finally mention should be made of two scripts of 3 November 1902, and 3 December 1902, both of which have *fatum ineluctabile*. She comments on neither in her MS notebook nor does she list either in *Proceedings*.

These are, I think, all the references to Books II and VIII of the *Aeneid* in any scripts of Mrs Verrall written before the publication in February 1903, of *Human Personality*. As early however as 1901 her scripts are emphasising an aspect of Rome, stressed indeed by Vergil, but not by Myers in his allusions to him, as a nation dedicated to peace (*gens togata*). This becomes more and more prominent in the scripts, and by the time of her remarkable script of 29 April 1907 (see *Proc.* 26, 182), it became plain to her and to anyone else who read it that all these Vergilian references led up to the notion of what that script calls 'a wider thing, a universal country, the mother of us all'. (But Jerusalem which is above is free, which is the mother of us all. *Galatians* IV, 26.) The Roman Peace, founded on force, is no longer by itself acceptable as a type of the new world order. The conception must be extended to include the ideals which are associated with Athens and Jerusalem. Nor is the conception, as with Myers, one of gradual progress over an indefinite extension of time, but a plan already in existence before the death of M. L. in 1875, promoted through the co-operation of a large number, including the seven communicators, who have survived bodily death, and their friends still in this life, and realisable within the not too distant future. May I repeat that I am interpreting not prophesying?

On pages 204-8 of her paper Lady Balfour points out the part played in the development of the scripts by the cross-correspondences and their relation to the evidence for survival of M. L. and other members of the communicating group. Had it not been for the element of complementary design to which Alice Johnson drew attention, it might have been held that the veridical personal references in the scripts, when they were not part of the conscious mental content of the automatist, were the product of subconscious dramatisation of latent memories. So with such a topic as the new world order, a conception likely to have made a strong appeal to idealists of the generation in which Mrs Verrall and Mrs Willett grew up. Such a cross-correspondence as that known by the phrase *Ave Roma Immortalis* (*Proc.* 21, 297-303), besides its interest as a separate incident, obviously links on to the Vergilian quotations already cited. As these take up Myers's parable of the foundation of Rome, and continue it to the Augustan period, so this cross-correspondence continues it still further, into the triumph of Christian Rome. Neither Mrs Verrall nor Mrs Holland, the two

automatists concerned, would, I think, have considered that event from the angle of theological dogma but as a type of the supersession of an order based on force and cruelty by a humaner one. It is probable from the same angle that one must regard the references in the Sevens case (*Proc.* 24, 243-54) to the Pageant seen by Dante in the Earthly Paradise (*Purgatorio*, cantos 27-33). But that case introduces concurrently the topics of the reunion of lovers, which has a special relevance to the Palm Sunday story, and of the number seven, the significance of which in relation to the whole scheme of the scripts is too obvious to need elaboration.

That there is a scheme of the utmost complexity in all the scripts of the S.P.R. group of automatists, a scheme set out through an elaborate use of symbolism and cross-correspondences, by means of which references are made, cryptically at first and from 1912 on more explicitly, to individuals and to such topics as the establishment of a better world-order and the way to bring it into being, I have no doubt at all. My opinion is based on a reading, often a re-reading, of all the scripts of Mrs Verrall, Mrs Holland, H. V., Mrs Stuart Wilson, and (with the exception of a few private scripts) Mrs Willett: I have also read a few scripts of Dame Edith Lyttelton, Mrs Richmond, and what are called the 'minor automatists', and the records of the Piper sittings connected with the scripts. Together they constitute the largest and the most intricate piece of material in the history of psychical research. It is fortunate that the investigating group were persons of exceptional acumen, patience and capacity for hard work, and that they received full co-operation from the automatists.

The success of an attempt to elucidate writings of this nature and quantity must be tested by whether the methods of interpretation produce coherent and consistent results. On the whole, so far as I can judge, the interpretation is both coherent and consistent, though there are some points, as the investigators admitted, of ambiguity, and others where I am not myself fully convinced. To what then do the scripts, as so interpreted, point? The prophecies must stand or fall by their fulfilment or failure. Signs of fulfilment are up to the present lamentably lacking. If however a time should come when it would be reasonable to regard them as having failed, their failure would not have any real bearing on the evidence for the communicators' survival. This all the investigators, including the highly critical Mrs Sidgwick, accepted as established.

Lady Balfour has therefore the best of authority for assuming that the 'communicators' are what they claim to be. If in this note I have preferred the non-committal phrase 'script-intelligence' it is not that I am playing for safety, or that I dissent from the view

that she and the investigators, with fuller knowledge than I can claim, have accepted. It is to a large extent a question of language. To many people the word 'personality' suggests something with clear cut boundaries. None of the investigators would probably have held this naïve opinion. Mrs Sidgwick in fact (see *Proc.* 33, pp. 420-3) argues for 'the partial merging of two minds' as the explanation of some spontaneous cases, and as having a bearing on communications through mediums. In the scripts of 'the S.P.R. group' it seems to me that this 'partial merging' or fusion has been carried to a degree in which the fusion is of more importance than the constituent parts. The Palm Sunday case furnishes good evidence that the 'communicators' continue to exist in some way. In my view they continue as constituents of a fusion which I call the 'script-intelligence'. This sentence is obscure enough to fit the language of the early scripts. Perhaps like them it may achieve explicit significance if ever psychical researchers should take up again those types of investigation on which the reputation of our Society was founded.

A TANGLE FOR UNRAVELLING

BY ROSALIND HEYWOOD

THE PALM SUNDAY CASE: New Light on an Old Love Story, by Jean Balfour. *Proceedings of the Society for Psychical Research, Research, Vol. 52, Part 189, February 1960.*

(At our request Mrs Heywood has kindly written the following summary of the Palm Sunday story as a guide to members who have recently joined the Society.—Ed.)

IN 1875, on Palm Sunday, a young girl, Mary Catherine Lyttelton, died. Her little-known love story with Arthur, first Earl of Balfour, is recorded in the new issue of *Proceedings*, together with further material which suggests its apparent continuation beyond the grave.

Those who nowadays remember Arthur Balfour will think of him as an aloof and austere philosopher and as Prime Minister from 1902 to 1906. There is no one alive who knew him as a sociable young man, who, though somewhat apprehensive of strangers, delighted in good talk, good music and the company of his friends.

It was in 1871, at Christmas time, at a ball at Hawarden, the William Gladstones' home, that he met Mary Lyttelton for the first time. And he fell in love with her, there and then. Very wisely. Not that she was a great beauty, though she had unusually beautiful hair. But Dame Edith Lyttelton, the wife of her brother, Alfred (the statesman and sportsman) was later to write of her: 'Love and sympathy streamed out from her. She was one of those people who charge the atmosphere with life when they appear.' She was also an exceptional pianist, and Balfour's love of music was very deep.

But Balfours were never precipitate, and the well-brought-up young women of the time did not meet their suitors even half-way. It was not until 1875 that he got as far as to speak of his deep feeling for her. Even then he did not actually propose. That he intended to do at the next opportunity. But there was no next opportunity. Immediately after that talk she fell ill of typhus and died, on Palm Sunday, shortly before her twenty-fifth birthday.

Though few were aware of it, for Balfour her loss turned the world to dust and ashes. He gave her sister his mother's beautiful emerald ring to be buried on her finger. He wrote to his friend, Edward Talbot, her brother-in-law, 'I think—I am nearly sure—that she must have grasped my feelings towards her . . . Now perhaps, when she watches the course of those she loves who are still struggling on earth I may not be forgotten. . .'. He never married. But every year until his death in 1930, unless prevented by affairs of State, he passed Palm Sunday in seclusion with her sister, in remembrance of her.

The suggestion made in 'The Palm Sunday Case' is that new light may be shed on this old love story by the independent automatic scripts written by four ladies, who do not appear to have known about the events they refer to. These scripts purport to contain evidence that Mary Lyttelton and Arthur Balfour's younger brother, Francis, who was killed in 1882 when mountain-climbing, were both very much alive, and that he was helping her to convey to her lover the knowledge of her enduring devotion to him.

The four automatists were women of almost ferocious integrity. They were Mrs Verrall, lecturer in classics at Newnham College, Cambridge; her daughter, Helen, later Mrs W. H. Salter; Mrs Fleming, a sister of Rudyard Kipling; and Mrs Winifred Coombe-Tennant, a well-known public figure. The two last used the pseudonyms Mrs Holland and Mrs Willett, as they felt it vital to keep their gift of automatic writing secret. The chief investigators and interpreters of the scripts were Arthur Balfour's brother,

Gerald (the second Lord Balfour), their sister, Mrs Henry Sidgwick, and a particular friend, Mr G. J. Piddington.

For the time being, owing to the very personal nature of the scripts, they kept them a strict secret, and bequeathed them, together with their own interpretation, to the present Countess of Balfour. Now that the last person directly concerned with the story is dead, she has prepared them for publication.

The scripts fall into two groups. The first, from 1901 to 1912, consists of cryptic allusive fragments, written by Mrs and Miss Verrall, independently, and by Mrs Holland, who lived in India. None of them appeared to know anything about the romance. Mrs Holland came into the group because one of her scripts itself told her to send it to Mrs Verrall's address in Cambridge. She did not know Mrs Verrall or Cambridge and had never consciously heard of the address.

The Palm Sunday fragments were interspersed with many others, which were meaningless taken alone, but which were ultimately considered by the investigators to make sense when pieced together, usually after a clue had been given in a script. About 3,000 scripts were written over thirty years by about twelve scattered automatists of the highest integrity, including Dame Edith Lyttelton and the four mentioned above. The scripts themselves claimed that three distinguished Cambridge scholars, Professor Henry Sidgwick, Frederic Myers and Edmund Gurney, who in life had been interested in the problem of survival and had recently died, had devised this method of demonstrating their continued existence and power to plan. They had discovered during their lifetime that to do this is extremely difficult. For a purported communicator merely to say 'I am Myers,' or 'I am Mary Lyttelton,' is no proof at all of identity. If he says: 'Do you remember this or that?' the answer is, 'Yes, and because I do, even if the automatist has not discovered it by normal means, it is simpler to say that she got it telepathically from my living mind than from the dead.'

But, said the script intelligences, here were they producing evidence of design which was not in any living mind. So where would that have come from? Mary Lyttelton and Francis Balfour were obliquely indicated as being members of the group creating the designs.

The clues which, in the view of the investigators, made sense of the scattered fragments in the earlier Palm Sunday scripts were not disclosed until 1912, when automatic writing by Mrs Willett took up the theme. She had already produced a number of scripts purporting to come from Myers and Gurney, and her automatism was now taking an unusual form. Sometimes she wrote, but some-

times she talked alternately to the investigators and to the script intelligences. These said that they were training her to do this as they did not wish her to become an ordinary trance medium, who loses consciousness entirely. They wished her to learn to poise her consciousness on a knife edge, so that she could become aware of what they said and yet retain enough contact with the exterior world to be able to report back to the investigators.

In 1912 her script insisted that Arthur Balfour's brother, Gerald, should come and sit with her while she wrote. He came, and the script intelligences, whoever they were, showed fine dramatic sense, for they chose Palm Sunday to begin to elucidate the mystery. The earlier scripts must be gone through again, they said. This was done, and, in the light of indications provided by the new scripts, the investigators came to the conclusion that for ten years the continued love of Mary Lyttelton for Arthur Balfour had often been alluded to in symbolic fashion.

Mary had been called in the scripts the Palm Maiden, or May Blossom, or the Blessed Damozel or Berenice. Most of these were clear enough. She died on Palm Sunday, her family called her May, even in Heaven the Blessed Damozel yearned for her lover. But why Berenice? She, it will be remembered, sacrificed her hair to the Gods for her husband's safe return from war. The scripts also contained scattered references to a candle and candlestick, a lock of hair, something purple, a metal box and a periwinkle.

The link between these was not apparent even to the investigators until 1916, when, in response to impassioned appeals by the script Arthur Balfour himself came and sat with Mrs Willett. In July she wrote a long and moving script, speaking of the presence of the May Blossom and referring to the various symbols and the lock of hair. She also wrote, among other emotional passages, 'And if God will I shall but love thee better after death.' This script was signed M. (Other early scripts had Mary Lyttelton's initials, and one of Mrs Holland's was signed Mary L., but this, of course, meant nothing to her.)

To the reader all this oblique allusiveness must seem pointless; but, in addition to the reasons for it given above, the script intelligences said repeatedly that using the brain of another person was extremely difficult, and that, if what they caused her to write became too meaningful to her, it was liable to arouse trains of her own associations and these would appear as misleading extraneous matter in the scripts. The above long allusive script, however, was quite meaningful to Arthur Balfour. He now told his brother, for the first time, that shortly after Mary Lyttelton's death, over

forty years before, her sister had shown him a lock of her beautiful hair, which had been cut off during her last illness, and that he had had made for it a silver box, which was lined with purple and engraved with periwinkles and other spring flowers.

This lock of hair incident is a typical instance—so it seemed to the investigators—of a later event throwing light on past scripts, written by people who knew nothing of the matters referred to. Another is the frequent use of the candlestick symbol for Mary Lyttelton by three automatists who apparently had no idea that a photograph existed in which she stood holding a candle at the foot of a staircase.

Naturally enough, Arthur Balfour was slow to commit himself about so basic a matter as the authenticity of the scripts, but gradually over the years he came to accept that the messages he received did indeed come from Mary Lyttelton, and they appear to have been a great comfort to him. In 1926, when he was old, and ailing after pneumonia, he wrote her a message in reply to an emotional script begging him to believe in her continued presence. This was to be read to Mrs Willett when she was in the dissociated state in which she wrote her scripts. 'The message,' he wrote,

is understood by him and is deeply valued. . . . Assuredly he does not require to be told that death is not the end. . . . The hour of reunion cannot be long delayed. During all this period he has had no access to her mind except through the rare intervention of others, no intuition of her presence, although he does not doubt its reality. Through his complete deficiency in psychic gifts he has no intuition of that 'closeness beyond telling' of which the message speaks with such deep conviction and which he conceives to be of infinite value. Further messages would greatly help. . . .

In October 1929 Arthur Balfour was living with his brother, Gerald, at Fishers Hill, near Woking. He was now eighty-one and very frail. Mrs Willett went there for a visit and while sitting one evening quietly with the two Balfour brothers and Mrs Sidgwick she fell into a half-dissociated state and had an experience which moved her profoundly. Lady Betty Balfour, Gerald's wife, wrote down what she said as she came back to normal consciousness. She began 'A. J. B.'s room was full of presences—such light, such radiance.' Lady Betty asked: 'What sort of presences?' The answer was:

One figure there—things coming out of that figure—such wonderful things. The profound unchangeableness of it all. It was a woman's figure—quite young, dressed in an old-fashioned dress—Lovely quantities of hair gathered round her head. . . . Impossible to describe all

the things that seemed coming from her—the mass of sureness, tenderness and power. It made everything else in the room appear dead while every form of life you can imagine radiated from her. Her hand was upon his arm—she never took her eyes from his face. I told G.W.B. that it seemed as if in a sense Time had vanished. When you have an experience like that you *know* masses that no mere words can describe. If only I could draw—a perfect line can sometimes express what no words can.

On the last occasion when Mrs. Willett's script appeared to refer to Mary Lyttelton she wrote, 'Tell him he gives me joy.' A few months later Arthur Balfour had a stroke, and died soon after it. Immediately before the stroke his nephew's wife, the compiler of this record, was with him, and outside in the passage a record of Handel's Messiah was being played. She wrote afterwards in her private diary that, suddenly, she had felt, as a blind person might do, that the room was full of a radiant, dazzling light.

Now it seemed to me that there were people there too. . . . I knew that they were clustered round A.J.B.'s bed. They seemed to me to be most terribly eager, very loving and strong. . . . I felt they were there for some purpose. . . .

Then she felt compelled to look at A. J. B.'s face, and saw it 'transfigured with satisfaction and beauty' as at a glorious vision. A moment later he had the stroke, to her great distress. 'But,' she wrote,

The extraordinary thing was that I was vividly aware that the feeling in the room had *not* changed, that the radiant joy and light still thrilled around him, that the agonising spectacle of the poor body's affliction caused no dismay whatever to those unseen ones who watched. As I ran for the Doctor I was saying to myself over and over again, 'It was intended, it was *intended*.'

So strictly had the secret of the scripts been kept that at that time the present Lady Balfour knew nothing of them or of the work of their interpreters.

In the Palm Sunday scripts there is a mass of material which appears to hang together and which also convinced a group of highly intelligent investigators, who gave many years to its study, that it referred to persons and events unknown to the scattered automatists who produced it. Where did it all come from? Given the characters of the persons concerned, conscious fraudulent collusion seems out of the question. Then can chance coincidence be stretched to cover it? If not, must we postulate extended extrasensory collaboration between the subliminal minds of upright persons, to deceive the conscious minds of themselves and

their friends? Or did it originate, as it claimed, with discarnate intelligences?

Material of this kind is liable to arouse vigorous reactions, and, unfortunately, it cannot be measured or repeated to order in the manner demanded by the physical sciences. As with testimony in a court of law, everyone must form his own opinion on it for himself.

THE JONES BOYS; THE CASE AGAINST CHEATING

BY S. G. SOAL

IN this article I summarise various arguments put forward in a talk which I gave to the Society on 20 January 1960 telling against the hypothesis that the boys obtained their high scores by the use of a high-frequency whistle or other means of deception.

I began these experiments by seating the boys one at each end of a 4' 3" long table with an adequate screen placed across its centre. I was fully alive to the possibility of the boys trying to signal by touching feet because in 1936-9 I had tested scores of students at University College London by this very method using a similar table and screen with a distance of only 4' between agent and percipient. As a matter of long habit my own legs were stretched out under the table which was not more than about 2 feet wide and being on the look-out and not a complete fool I should have been quickly aware if they were signalling under the table. I began in this way to inspire confidence before increasing the distance apart.

When on 25 September Mrs Goldney, Mr Bowden and I moved Glyn from his position at the table into an adjoining room *without giving the boys the slightest warning* Glyn made even better scores.

In all the indoor experiments of April 1956 at which Mr Bateman was present the boys' feet were completely screened whether they were in the same room or in different rooms.

The first London visit

In a lecture to the Society in the autumn of 1959 Mr Hansel tried to show by photographs etc. that Ieuan could have signalled to Glyn when the boys were performing in the S.P.R. library by

protruding a toe slightly outside the screen of index boxes which filled the space beneath the table behind which Ieuan sat. He forgot however to mention that when Professor Mace was present on 12 October 1955 the latter watched Ieuan and myself the whole of the run in which Glyn made a score of 18/25. During this run Mace was in full view of the end of the index box screen (see Fig. 4, p. 60 *The Mind Readers*) and could not fail to have noticed any signalling by Ieuan's feet. Mace afterwards wrote saying that he noticed no cheating but then he would not have seen the trick if he had watched a conjuror's performance. Mrs Goldney, also, would probably have noticed Ieuan's foot protruding outside the index boxes. Most of Mr Hansel's audience were unimpressed by his demonstration.

The High Frequency Whistle

In the experiment of Whit Monday 1956 held in St Paul's school playing-field Glyn sat behind a very high and very wide screen making high scores at distances of 69-99 feet. It is true that an elder brother Gareth Jones was acting as signaller in this latter part of the sitting but he had never been signaller in any previous experiments and had absolutely no reason to anticipate that he would be called on to relieve Mr Fisk on this occasion. In fact he did so at the request of Mr Fisk who wished to watch Ieuan for a time. Gareth is a boy of excellent character and is now an engineer in television sets, highly thought of by his employers, and is moreover possessed of a good scientific intelligence. There is no reason whatever of suspecting his probity.

In the experiments of August 1956 Glyn sat behind an adequate blanket screen suspended from a line and persons like Dr Humphreys (Biologist), Professor Mundle and others with good honours degrees in science all testified that no visual signals or ordinary auditory cues were possible.

At the back of the house in which Glyn lived there is one small bedroom window; the other small window belongs to the adjoining house occupied in 1956 by a non-member of the family.

The diagrams on the scoring sheets show clearly that in the *great majority* of these open-air experiments Glyn sat at a small table with his *back* to the house so that he certainly could not have received *visual* signals from the small window without having to turn his head and look upwards constantly during the experiment.

We are left therefore with the possibility that the boys were employing either a midget radio set or some kind of high frequency whistle with a code of 0-4 or 1-5 pips for the five symbols. It is absolutely certain that the high scores produced in near-darkness

at a distance of 100 feet with Professor Mundle watching Ieuan and Dr Whitehead acting as signaller could only have been produced normally by one of these instruments.

The difficulties of up-keep and the skilled attention which midget radio apparatus requires rules out its use in the opinion of experts in these matters.

But one critic has outstripped all others by the suggestion that the boys signalled electrically by means of *underground* wires ending in small terminals just visible above the turf of the pitch! But since Glyn was being frequently moved into different positions—not usually arranged by himself—this suggestion falls very flat.

But as regards the high frequency whistle it is highly improbable that the boys had ever heard of such a thing.

The headmaster of Llanrwst County School which Glyn attended informed me by letter that the science department there does not possess such an instrument and that Glyn would neither hear nor see one at the school.

Another man who was headmaster of the village school at Capel Carig for many years and later of the school at Bettws-y-Coed signed a statement that in all his experience he had never once found boys playing with a sheep-dog whistle and he doubted if any of the boys in the neighbourhood had heard of such a thing. I have talked to several shepherds in the district and though certain of them know about the existence of such whistles they do not themselves actually use them in their work.

I did however in the early autumn of 1958 learn that there was some years ago a small shop at Llanrwst seven miles away from Capel Carig where the 'Acme Silent Dog Whistle' could have been purchased. (For that matter it is sold at Gamages.) The shop had been closed for some time. Glyn and Ieuan had little connection with the sheep-farmers of the district, belonging as they do to the mining community. In any case the Acme Whistle is unsuitable for telepathy experiments since it is easily heard by a great many people over forty.

In my own experience with a Galton whistle costing £7 and furnished with a micrometer screw by which the pipe-length can be adjusted to 0.1 millimetres, I found that boys of from say 15–17 years of age could hear the note when the instrument had a pipe length of 3 mm. or more when they were a few feet away and it was in my trouser pocket. But they did not usually hear it when set to less than 3 mm. even when in the same room with it. At 3 mm. it could be heard with some difficulty up to about 120 feet. But then I found that in a room at Oxford 22 out of 26 young men most of them over 20 years of age heard it quite as well as the boys when

set at 3 mm. Quite a large percentage of men of 30 and slightly over hear it as well as do 15-year-old boys. Mr O. W. Owen, I found in 1959, could hear the Galton whistle when set at 3 mm. as easily as Glyn and I think he would have heard it at the three experiments which he attended (31 July, 4 Aug. 1956 and the radio experiment of March 1959). But very few people over 50 can hear even the Acme whistle when it is screwed up to the highest pitch.

When Mr Scott came to my flat to give a demonstration of pseudo-telepathy I heard nothing at first but noticed that he was holding his hand to his breast and I asked if he were using a midget radio set. But on several occasions later on the same evening though I could not detect the supersonic note I could hear distinctly the air in the bulb even when the whistle was inside his clothing. And I think Mr Bowden would have noticed this sound after a short time sitting as he did directly in front of Ieuan the agent. In any case I do not think the boys could have kept up the deception very long.

It has been said that moving the agent who uses the whistle out of alignment behind a door or wall cuts off the sound from the listener. But this is not true if there are facilities in the immediate vicinity for numerous reflections. As I pointed out in the *Journal* (December 1959) the Galton whistle set at 3 mm. could be heard by Glyn sitting in the living room of his house when it was sounded from positions in the adjoining café quite out of line with Glyn and the closed and latched door. I came to similar conclusions by testing another 15-year-old boy in the corridor outside a hotel room. Much depends also on how closely the doors fit. When I live in Wales I hope to do further experiments with both Glyn and Mr Owen after having calibrated the Galton whistle.

I think further that if Glyn had been using a whistle in the experiments carried out on October 15 1955 in the S.P.R. library when Dr West was present the sound would probably have been noticed by the 13-year-old Christopher Yolland who was in the room the whole time. At this sitting scores of 16 and 17 were made. Ieuan and others certainly heard the whistle quite distinctly at Mr Scott's television demonstration in April 1959.

The New Cards Experiment

In this test held on the morning of 7 April 1956 cards bearing five different coloured pictures were sprung upon the boys without a moment's warning. The pictures were (i) Chelsea pensioners in scarlet coats with S for Soldier inscribed on the face; (ii) A

dark-blue coated policeman bearing the letter M for Man; (iii) A racing car painted a bright yellow and inscribed R; (iv) A black print showing St Paul's Cathedral and marked L for London and (v) a black print of Prince Charles marked B for boy.

During the experiment Mr Bateman was in a position to observe Ieuan's feet without obstruction and he is certain that no visual signals nor ordinary auditory communication between the boys was possible. Any code used would have had to be an ultrasonic one.

The numbers of times the different symbols were guessed and the numbers of hits on each symbol are given below.

Symbol	B	L	M	R	R	Totals
<i>Times guessed</i>	40	35	48	34	43	200
<i>No. of hits</i>	21	17	16	21	15	90
<i>Expectations</i>	18.00	15.75	21.60	15.30	19.35	90

We have $\chi^2 = 0.368$ and $P > 0.05$ which is not significant.

The percentage Hits/Guesses for each symbol are:

<i>Black print</i>	B	52.5%
<i>Black print</i>	L	49%
<i>Dark-Blue coated</i>	M	33%
<i>Yellow</i>	R	62%
<i>Soldier (Red)</i>	S	35%

A critic has suggested that the boys might have prepared for such a contingency by memorizing a code of 'pips' based on colour. This seems most improbable. If for instance they had agreed beforehand that a red object such as S should be allotted the same number of pips as the familiar red *lion* we should have expected them to plump for Soldier and score heavily on it but actually they obtained only 35% of hits on this card. Similarly the dark-blue policeman (M) might have been identified with the dark-blue penguin of old but on M they obtained only 33%, the lowest score of all. Further it would have been difficult to distinguish on the basis of colour alone between L and B whereas they actually scored highly on both symbols.

The 5-5 table below shows the number of times each symbol was presented and guessed.

The figures in the leading diagonal show the numbers of hits on each target. Outside this diagonal two scores are exceptionally high and it will be seen that when the target is Soldier there are 12 guesses at Man=M and that when the target is Policeman=M there are also 12 guesses at Soldier=S. As these two figures are almost comparable in order of magnitude with 15 the lowest score in the leading diagonal it seems probable that there is

a genuine tendency to mistake Soldier for Policeman and Policeman for Soldier; they are both men in spite of the different colours of their coats. If however we reject the ESP explanation we must assume that probably the boys were using the same number of pips for both targets.

TARGETS

GUESSES		B	L	M	R	S	TOTALS
	B	21	4	6	4	5	40
	L	5	17	2	4	7	35
	M	10	6	16	4	12	48
	R	1	6	5	21	1	34
	S	3	7	12	6	15	43
	TOTALS	40	40	41	39	40	200

It has been suggested that the boys might have arranged beforehand to assign the numbers 1-5 for any fresh set of letters taken in alphabetical order—in this case in the order B₁ L₂ M₃ R₄ S₅.

Now a person who listens to the whistle seldom hears *more* pips than are actually sounded. He tends to miss one or two pips e.g. to say R when the letter is S or B when it is M. 5 pips would be frequently heard as only 4; yet the table shows this happens only once! Actually Glyn mis-hears pips by deficiency 49 times and by excess 61 times and there is no significant difference between these figures. On the alphabetical theory then, we must conclude that if Glyn uses a whistle he hears it almost perfectly and this is confirmed by our experiments of September 1959 (see *Journal* Dec. 1959). But of course Ieuan might have made many mistakes through his failure to master the code. But the comparatively low scores on M and S strongly suggests that if a whistle was used the same number of pips was employed to represent these symbols. But if this is the case it would not have been sufficient for the boys to agree that any new symbols should be taken in alphabetical order but in addition Ieuan would have had to remember to use the same number of pips for the third and fifth symbols. This seems to me an incredibly complicated supposition.

Glyn started his first scoring sheet by writing in places 1-5 the letters M R S L B. If this was intended to be the code the reader may verify by constructing the corresponding 5-5 table that we are led to precisely similar conclusions and the same complications. In this case Glyn mis-hears the pips 55 times by excess and

55 times by deficit. Actually on the last column of this sitting when the code is presumably mastered he started off with L M S R B so that not much notice need be taken of this possibility.

On the *first* column of this sitting Glyn got 10/25 and five consecutive hits R B S L M in places (18-22). Yet in the second column he makes a worse score 8/25 than on the first! There is in fact very little to suggest that any code was employed in the erratic variations of success. For instance, in the last column the score drops to 7/25.

Mr Hansel has suggested that, in the outdoor experiments, the whistle might have been worked, not by Ieuan, but by another member of the family sitting behind the small upstairs window and watching slight movements of Ieuan's feet. The only person I can imagine doing this efficiently would be Richard Jones. On the day when I searched Ieuan and found nothing Richard was away at work. I know this because, living myself in the house, I was well acquainted with the movements of all members of the family and on that day Richard left for the lead-mine fully an hour before the sitting began. Gareth was also all day in the assaying office of the mine. But it is extremely dubious if Ieuan's feet could be seen clearly by a person standing at this window, as I have tested for myself. In summer there is on this part of the pitch tall bracken on either side of the path and also small depressions in the ground which would obscure the view. Actually the observers were especially asked to watch Ieuan's feet (see *The Mind Readers* p. 130), and in the summer of 1956 I used to insist that Ieuan sat well back from the low improvised table so that Bowden and the observers could watch his feet. A motion of a foot occurring with regularity at every guess would surely have been noticed.

On the day when Ieuan and Glyn were searched the former remained standing close to Bowden who watched him till he was joined by C. W. S. the signaller. If Ieuan had taken something out of his bathing dress and dropped it on the very short thin turf, Bowden would certainly have noticed it.

At another of these open-air sittings a violent gale was blowing from Glyn to Ieuan and I very much doubt if a whistle could have been heard by Glyn whether it was blown by Ieuan or by a hypothetical helper stationed inside the house. But this I shall test when opportunity offers.

Important Failures

Some of the more striking anomalies which make the 'supersonic' theory improbable are the vagaries in the boys' failure to

score above chance level in the Easter 1957 visit to Wales of Mr Bowden, Mr Gliddon of Queen Mary College and myself. The boys were clearly expecting to rake in the shekels during this visit for Ieuan had postponed starting his new job at a local hotel for a whole week so as to be free to do experiments. Glyn was on holiday. Notwithstanding this excess of enthusiasm one experiment after another in the open-air failed to produce a single significant score. The boys got on very well with Mr Gliddon who accompanied them on excursions and had no reason whatever to distrust him, and the other conditions were similar to those which obtained in 1956. Even when Richard and I were the only persons present they still failed to score.

But strange to say at noon on Sunday April 14 they suddenly made high scores at an indoors experiment held in Ieuan's house. At this sitting they made scores of 10, 12, 13, 16, 11, 15, 10, 15.

The boys were in adjoining rooms and quite out of direct line with the open doorway so that the line joining them passed through two solid walls (see *The Mind Readers* p. 212.) Gliddon was satisfied that visual signals and ordinary auditory cues were ruled out as impossible. Even Mr Hansel can only suggest that a high frequency whistle was used. No other member of the Jones family was present or even in the vicinity of the house so that if a whistle was used it must have been by Ieuan. By later experiments I did verify that Glyn *could* have heard a whistle sounded by Ieuan under these precise conditions by means of the numerous reflections.

The very next day we carried out an experiment in Glyn's house under somewhat easier conditions but this again was a failure. Later in the evening of the same day under good conditions of screening the boys made a significant score of 64/225 ($CR=3.16$) which earned for them just two shillings apiece! Obviously if there was a whistle they still had it and made singularly poor use of their opportunities. Again it is hard to understand why having made scores of 21, 20, 18, 19 on the morning of 28 August 1956, with Mr Salvin, Dr West, Mrs Goldney, Mr Reeves all present, their scores were only 3, 4, 6, 3 on the afternoon of the same day with only Mr Salvin, Mrs Goldney and Bowden present excluding the fathers who were there on both occasions.

As regards the second successful clairvoyance experiment of 17 August 1955, in which the card to be guessed was covered by a larger rectangle of cardboard I can only say that I have demonstrated the method to several psychical researchers and not one of them has been able to catch a glimpse of the covered card. I used this method for years at University College, London, and not a single

Society for Psychical Research

1 Adam & Eve Mews · London · W 8

JOURNAL, JUNE 1960, VOL. 40, No. 704

SUPPLEMENT No. 25

FOR MEMBERS AND ASSOCIATES

ANNUAL GENERAL MEETING

THE Annual General Meeting of the Members of the Society was held at the English Speaking Union, 11 Charles Street, W.1 on Saturday, March 26th, 1960 at 3 p.m., under the Chairmanship of the President, Professor C. D. Broad. 36 members and associates were present.

The Secretary having read the Notice convening the Meeting, the Report of the Council was presented. The President said that there was an omission on page ii in the Report of the names of 27 Members who had been removed for non-payment of subscription. There was, therefore, a net decrease of two in the membership figures for the year, leaving a total of 1,114. Miss Saville raised questions concerning the attendance figures of Members of Council, the regulations relating to nominations to Council and the names of co-opted Members of Council. Mr Salter answered her enquiry regarding the position of Miss Green, the Secretary (Research), and her future activities. Mr Lambert, the Convenor of the Anonymous Donor's Fund Committee, explained the status of the Committee. It was agreed to publish a list of Honorary Associates and Corresponding Members in a Supplement to the Journal which would give a Report of the Annual General Meeting. A list of home, overseas and American Members would be published in Proceedings during the year. Mr Sproull drew attention to the absence of research in the field of physical phenomena. Mrs Goldney pointed out this was because there were no mediums willing to submit to investigation. Mr Sproull mentioned that he had suggested to the Research Committee that some of the Society's funds could be used to finance an investigation into the literature in order to obtain data as a basis for the Society to train its own physical mediums. Mr Scott reported that the committee had promised to prepare a list of points to be followed up in physical phenomena research. After further discussion in which matters relating to research, the system of interviewing and the Library were raised by Miss Saville, Mr Stark, Dr Dingwall, Mr Gilbert and Mr Edmunds, Mr Lambert moved the adoption of the Report. This resolution was seconded by Mr Cutten and carried unanimously.

In presenting the Accounts, the Hon. Treasurer, the Hon. Charles Strutt, commented that they covered a full year and not nine months as in the previous year. He thought the investment position had improved with the help of the Society's professional advisers. In answer to questions from Miss Saville, he gave details of the terms of the bequest of Albert Hunt and of the other legacies received during the year. Mr Scott replied to Miss Saville's question regarding the settlement of a threatened libel action. Dr Dingwall asked about a payment of £25 to Sir Rudolph Peters for an investigation. The President said Sir Rudolf would be asked for a Report on this matter. Mr Salter, seconded by Mr Oram, proposed the adoption of the Accounts. This resolution was carried unanimously.

Mr Oram proposed and Dr Fairfield seconded a resolution that the six retiring Members of Council should be elected en bloc. This resolution was passed by a vote of 22-7.

Mr Cutten, seconded by Dr Smythies, proposed that the following six Members of Council should be re-elected: Mrs O. Gatty, the Hon. Mrs K. Gay, Dr S. G. Soal, Prof. F. J. M. Stratton, Dr R. H. Thouless, and Dr D. J. West. This resolution was carried by 25 votes to 4.

On the proposal of Miss M. T. Saville, seconded by Mrs K. M. Goldney, Messrs Deloitte, Plender, Griffiths & Co. were re-elected Auditors for the forthcoming year.

Mr W. H. Salter moved a vote of thanks to Professor Broad for his work as President during the past two years.

Members' questions regarding the Journal, the housing situation and other administrative matters were answered by the Honorary Officers.

MEETINGS OF THE COUNCIL

Meetings of the Council were held as follows:

549th	18 Jan. 1960	Chairman: The President, Professor Broad.
550th	15 Feb. 1960	Chairman: The President, Professor Broad.
551st	26 Mar. 1960	Chairman: The President, Professor Broad.
552nd	26 Mar. 1960	Chairman: The President, Professor Broad.

At the meeting of the Council held immediately after the Annual General Meeting on 26th March 1960, the following were elected:

PRESIDENT

Professor H. H. Price, F.B.A.

HONORARY OFFICERS

Hon. Treasurer, The Hon. Charles Strutt.

Hon. Secretaries, Mr Cutten, Sir George Joy, Mr Lambert.

Hon. Supervisor of Experimental Research, Dr West.

COMMITTEES

Editorial Committee: Mr Fisk (Convenor), Prof. Broad, Mr Lambert, Mr Oram, Mr Parsons, Prof. Price and Mr Salter.

Finance Committee: The Hon. Charles Strutt (Convenor), Admiral Strutt, Mr Lambert, and Mr Oram.

Library Committee: Sir George Joy (Convenor), Mr Lambert, Mr Manning, Sir Clifford Norton, Prof. Stratton, Mrs Tickell and Miss Green.

Research Committee: Dr West (Convenor), Mr Cutten, Dr Fairfield, Mr Fisk, Dr Fletcher, the Hon. Mrs Gay, Mrs Goldney, Mrs Heywood, Mr Oram, Mr Parsons, Mr Scott, Dr Smythies and Dr Soal.

House Committee: Mr Lambert (Convenor), Mr Cutten, Sir George Joy and Mrs Beale.

Anonymous Donor's Fund Committee: Mr Lambert (Convenor), the Hon. Mrs Gay, Mrs Heywood and the Anonymous Donor.

CORRESPONDING MEMBERS

CORRESPONDING MEMBERS

Professor Chari	Professor Gardner Murphy
Professor C. J. Ducasse	Mr J. Fraser Nicol
Professor F. Egidio	Dr J. B. Rhine
Mrs E. Garrett	Professor E. Servadio
Dr T. N. E. Greville	Admiral A. Tanagras
Dr G. H. Hyslop	Dr W. H. C. Tenhaeff
Dr Raynor Johnson	Dr R. Tischner
Dr C. G. Jung	M. R. Warcollier
Count C. Klinkowstroem	Dr T. Wereide
Herr Rudolf Lambert	Dr R. Wilson
M. Gabriel Marcel	Dr C. Winther
Dr C. A. Meier	Mr G. Zorab

HONORARY ASSOCIATES

Mrs Whately Carrington	Dr L. Rhine
Miss H. Carruthers	Mrs Z. Richmond
Mrs L. A. Dale	Professor C. M. Sage
Dr G. de Boni	Mr B. Shackleton
Mr G. W. Fisk	Mr G. H. Spinney
Mrs Osborne Leonard	Dr R. H. Thouless
Dr B. Humphrey Nicol	Miss N. Walker
Miss M. Phillimore	

MEETINGS OF THE SOCIETY

Private Meetings at Kensington Town Hall

Wednesday, 20 January 1960, at 7 p.m. Dr S. G. Soal, M.A., D.Sc.:
'The Jones Boys Experiments—the Case against Cheating'.

Wednesday, 10 February 1960, at 7 p.m. Mr G. Zorab:
'Relationships in Spontaneous Paranormal Phenomena'.

Wednesday, 16 March 1960, at 7 p.m. Mr H. Norman Hunt:
'Recordings from Spiritualist Mediums'.

NEW MEMBERS

MEMBERS

(Elected 18th January 1960)

COOMBE-TENNANT, A. J. S., B.A., 18 Cottessmore Gardens, London, W.8.

FORD, R. E. M., Berliner-Allee 40v, Dusseldorf, W. Germany.

HAVAS, F. de, Holmbury House, Holmbury St. Mary, Surrey.

KISS, G. R., 125 Rodenhurst Road, London, S.W. 4.

OLSON, A. D., 1 Kilmorey Gardens, St. Margarets, Twickenham, Middlesex.

THOMSON, G. McG., 103 Mendip Crescent, Westcliff on Sea, Essex.

STUDENT-ASSOCIATE

(Elected 18th January 1960)

STEPHENSON, C. J., 'Cedar Ridge', St. Johns Hill, Woking, Surrey.

MEMBERS

(Elected 15th February 1960)

DOWNES, Mrs V. J., 148 Grove End Gardens, London, N. W. 8.

HALL, T. H., J.P., F.R.I.C.S., 8 North Park Road, Roundhay, Leeds 8, Yorkshire.

PINKERTON, V., A.M.I.E.E., 15 Salisbury Avenue, Swanley, Kent.

ROWE, J. W., M.A., Ph.D., Little Close, 127 Armour Road, Tilehurst, Reading, Berkshire.

STAFF, Miss V. S., B.A., 4 Montenotte Road, London, N. 8.

SUNDERLAND, Dr A., F.F.A.R.C.S., 18 Bathurst Walk, Iver, Bucks.

(Elected 26th March 1960)

BENTLEY, D. M. W., St. Margaret's Road, London, E. 12.

BROOKES-SMITH, C. H. W., 4 Crescent Road, Wokingham, Berkshire.

HULL, A. J., B.A., 52 Hafod Road, Hereford.

PAGE, R. A., East Nursery, Oak Avenue, Hampton, Middlesex.

PATERSON, T. N., Via Zezion, 5, c/o Casale, Milan, Italy.

SAI, Siddheshwar, B.A., LL.B., At P.O. & District, Balangir, Orissa, India.

STUDENT-ASSOCIATES

(Elected 26th March 1960)

GIBLIN, R. A., 4 King Edward Avenue, Dartford, Kent.

STANFORD, J. P. B., Mount Salus, Dalkey, Co. Dublin, Ireland.

person succeeded in making a significant extra-chance score by means of it.

Finally Dr R. H. Thouless has very kindly pointed out to me that if, in the new cards experiment, the boys were using a Morse code, the three pairs RL, SB, SR would be most likely to cause confusion, while the five pairs SM, MB, MR, ML, BL would be the least often confused. Turning to the 5-5 table I find the average number of errors for the first group is $25/3 = 8.3$ per pair and that for the second group it is $62/5 = 12.4$ per pair. Thus the hypothesis is contra-indicated. A similar method may be applied to the symbols E, G, L, P, Z and so far as I have gone there is no evidence of such a code.

NOTES ON SOME CRITICISMS OF THE SOAL-GOLDNEY EXPERIMENTS

BY CHRISTOPHER SCOTT

THE Soal-Goldney experiments with Basil Shackleton (1) are often regarded as offering the strongest existing evidence for extra-sensory perception. For this reason they have attracted a great deal of attention from sceptics, and in recent years a number of hypotheses have been advanced to account for the results in normal terms. As the experiments ended in 1942 and are not repeatable, such hypotheses are inevitably to some degree speculative. Nevertheless it is possible to obtain some new evidence supporting or opposing some of them even after so many years have passed.

This article brings together two separate notes (written at different times) on some investigations I have made into various non-psi hypotheses advanced by two critics of the Soal-Goldney experiments. It will be seen that no conclusions of crucial importance were reached, although the findings do perhaps reduce by some small amount the area of speculation attaching to the hypotheses examined.

Herr Rudolf Lambert's Hypotheses

In a letter to Mrs Goldney dated 24 December 1953, Herr Rudolf Lambert suggested that the Soal-Goldney experiments were defective in that they did not rule out fraud, and he outlined three hypothetical fraudulent methods whose occurrence, he states, would not be incompatible with the experimental controls as described in the Soal-Goldney report (1).

Before summarising these methods it is perhaps advisable to state that Herr Lambert advances them with a great deal of reserve. He is at pains not to *accuse* anyone of fraud; his intention is merely to show that these frauds *were not ruled out* by the experimental controls. He stresses the extreme improbability of the fraud hypothesis, but appears to regard the hypothesis of precognitive telepathy as yet more improbable.

The three methods are these:

1. Fraud by P and A alone. This would be accomplished by card substitution by A, in the manner first suggested by Mr C. E. M. Hansel and briefly described in 'Modern Experiments in Telepathy' (2); see also (3).

Lambert suggests this technique for Sitzings 27, 28 and 34, in which the Prepared Random Numbers (PRN) were not prepared by the experimenter.

2. Fraud by P, A and the chief experimenter (henceforth termed 'X'). Here X either (a) notes in advance a selection of target numbers in the PRN, or (b) alters a selection of the target numbers (of which he is the sole recorder) in the counters experiments, having, in advance of the sitting, given the required information about these targets to A and P. P must memorize pre-arranged guesses to correspond to these selected targets, and A must arrange the cards in his box in a pre-arranged order.
3. Fraud by P and X. This technique would apply only to the last three sheets (Nos. 5, 6 and 7) of Sitting 16. Here the digit/symbol code was partially fixed in advance in the sense that only four possible codes were offered to A, who was required to select one for each of the three sheets. This rules out radical card substitution. However, the four codes offered overlapped considerably and a single code can be written down which, while not itself one of the four, coincides on one or more symbols with each of the four. Using this code, P can take advantage of foreknowledge, supposedly provided by X, of a selection of the digits in the PRN. The assumption of this method is, of course, superfluous in that Method 2 is already available for this sitting. However, Method 2 involves A in the fraud, and the A for this sitting¹ only came to one other sitting. Thus, for economy of hypothesis (i.e. to reduce the size of the conspiracy) it might be thought that for this sitting Method 3 would be less implausible than Method 2.

¹ There were in fact two simultaneous A's in this part of the sitting, but we are concerned here only with the one who was successful.

Hypotheses 1 and 3 each lead to certain predictions regarding unpublished features of the data. Lambert in his letter asks for an investigation of these predictions. It seems that, for one reason or another, this request was never attended to at the time. However, Mrs Goldney and I have recently undertaken the necessary examination of the Shackleton records (copies held by the Society for Psychical Research) and the purpose of the present note is to report the findings. The question whether Lambert's hypotheses are consistent with the *experimental procedure* does not fall within the scope of this note.

Method 1

Lambert supposes that during the last few trials of each sheet A would be busily occupied computing the optimum card substitution, and this would preclude his continuing to score non-chance hits during this period. Lambert therefore suggests comparing the number of hits in the last one-third of the right-hand run (the 'b' run) on each sheet with the score on the remainder of the *b* run. This comparison is made in Table 1.

TABLE 1

Agent	Sitting	Sheet	Number of Hits	
			First 16 trials (+ 1) of col. b	Last 8 trials (+ 1) of col. b
R. E.	27	1	6	2
		3	8	1
		5	3	4
		7	3	1
R. E.	28	1	7	2
		3	6	1
		5	5	2
		7	5	1
Total R. E. (Chance expectation)			43 (25·6)	14 (12·8)
			First 15 trials (+ 2) of col. b	Last 8 trials (+ 2) of col. b
J. Al.	34	1	7	4
		2	5	2
		3	4	3
		4	4	3
Total J. Al. (Chance expectation)			20 (12·0)	12 (6·4)
Note. The clairvoyance sheets of Sitzings 27 and 28 are excluded.				

It will be seen that for the agent R. E., Lambert's hypothesis is confirmed. In the last 8 trials the number of hits is close to the chance expectation, while in the first 16 it is much greater (to a degree that is easily significant). Analysis shows, however, that this distribution of the total of 57 hits between the two sections of the run could well have occurred by chance ($\chi^2=2.274$, 1 d.f., with Yates's correction; $.2 > P > .1$). Thus the evidence in support of Lambert's hypothesis, though positive, is far from strong. For the agent J. Al. the hypothesis is not confirmed; the scoring rate is closely similar in the two sections of the run. Further, the score of 12 for the last 8 trials could hardly be due to chance ($P=.017$, single-tailed, exact probability computation).

In evaluating this evidence we must bear in mind that Lambert's prediction does not follow of *necessity* from his hypothesis: a clever enough A (and Lambert's theory requires considerable skill at least of J. Al.) *might* achieve some non-chance hits during the last one-third of the *b* run. All in all it seems fair to say that the balance of the above evidence is neither markedly in favour of, nor markedly against Lambert's hypothesis.

Method 3

The four codes offered to A are shown in Table 2 (taken from the Soal-Goldney report, p. 61).

TABLE 2

Code No.	Digit				
	1	2	3	4	5
I	L	G	P	Z	E
II	L	Z	P	E	G
III	Z	G	P	L	E
IV	Z	P	G	L	E

It is clear that P's optimum assumption is the following master code:

I	2	3	4	5
L or Z	G	P	L	E

Now if we suppose that P made this assumption and knew the random number sequence, we can predict the number of his successes when each of the four codes is used. In Code I, for example, he will always be right on digits 2, 3 and 5 and never on 4, while he will succeed on 1 if and when he assumes L rather than Z. It seems unrealistic, however, to suppose that P would have learned *all* the random digits (150 in all). We assume therefore that he learned only some of them, say N for each sheet, and relied on

guessing for the remainder. Lambert suggests 17 as a likely value for N. But even without a knowledge of N, certain conclusions follow, and Lambert makes the following predictions.

1. The worst code II was not used.
2. Sheet 6 with its 21 hits coincided probably with the best code III.
3. The experiment where code I was used showed a striking want of hits falling on Z (and also a certain want of hits on L).
4. The experiment with code IV showed a striking want of hits on G and P.
5. The experiment with code III probably showed only a small number of hits on Z, though this could have been compensated by an excess of chance hits on Z during the period of chance guessing.

TABLE 3

Sheet No.	Code No.	Digit					Total	
		1	2	3	4	5		
5	III	Code	Z	G	P	L	E	48
		Targets	7	12	11	9	9	
		Hits	3	4	—	5	6	
6	I	Code	L	G	P	Z	E	48
		Targets	10	9	11	7	11	
		Hits	4	3	5	2	7	
7	II	Code	L	Z	P	E	G	48
		Targets	4	11	12	12	9	
		Hits	—	3	4	7	4	
Note. All data except sheet numbers and totals are previously unpublished and presumably unknown to Lambert.								

The relevant details of the scores for the three sheets are shown in Table 3. The following are the results of a check on Lambert's predictions. Instead of dealing directly with the number of hits we have worked with scoring rates, that is, the ratio of hits to targets. This seems clearly appropriate.

1. False.
2. False.
3. Striking want of hits on Z: true. Scoring rate on Z: .29; average on all other symbols: .46.

Some want of hits on L: true. Scoring rate on L (.40) lies between that on Z (.29) and the average of the other symbols (.48).

4. Not applicable. Code IV was not used.

5. False. Scoring rate on Z (.43) is higher than on the other symbols (.37).

I have considered Lambert's predictions, here, as he in fact made them. However, his predictions quite fail to take into account the chance variation both in the number of appearances of each digit and in the number of hits obtained by guessing. Such variation might be expected to mask completely any effects such as he predicts. Indeed even if his hypothesis were true he would be remarkably lucky to find all his predictions successful. For this reason the above results can provide only the weakest grounds for supporting or rejecting his hypothesis.

Arguing after the event we can test Lambert's hypothesis better by ignoring his predictions. Examining Table 3 and the master code, we see that in Sheet No. 7 the maximum number of 'fraudulent' hits was the four on symbol P (digit 3). In this sheet we are then left with a further 14 hits to explain by chance, out of at most 44 trials. This involves a probability of .044 (single-tailed, exact probability computation). This coincidence is in itself difficult to accept and becomes even less plausible when we note that for Sheet No. 5 *no* hits were scored on symbol P (digit 3). Thus the same symbol would have had to score *all* the four 'fraudulent' hits on one sheet and *none* on another. This implies a fortuitous change of plan happening to suit the change of code, and it is just this type of explanation which Lambert's hypothesis is designed to avoid.

Summing up, the evidence is markedly unfavourable to Lambert's hypothesis 3.

Conclusion

Lambert has suggested three methods of fraud in the Soal-Goldney experiments. The data have been re-examined at his request. Method 1 is neither indicated nor contra-indicated by the evidence to any important degree. Method 2 cannot be tested. Method 3 has been found in rather strong conflict with the evidence. It must be remembered that Method 3 is relevant to only one part of one sitting and is essentially marginal to Lambert's broader hypothesis of fraud.

C. E. M. Hansel's Hypothesis of Card Substitution

Mr Hansel's paper (3) suggesting card substitution as a method of cheating in the Shackleton (1) and Stewart (2) experiments was shown to me in draft form when I was acting as Supervisor of Experimental Research for the Society. The paper was couched in speculative terms and I considered it my duty to arrange an

experimental trial of the method of cheating proposed, with an approximate duplication of the original experiments, in order to determine how far it seemed practicable and what sort of demands it made on the conspirators.

I therefore invited Mr Hansel to London and asked Mrs Goldney and Dr West to meet him at my home for a discussion and demonstration. Mrs Goldney was, of course, the junior experimenter in the Shackleton investigation and Dr West had also been present as observer at one of the Shackleton sittings.

The following account is an extract (slightly edited) of the notes I wrote a few days after Mr Hansel's visit on 5 July 1958.

The main purpose of the meeting was to arrange an actual demonstration of the card substitution method of cheating in conditions similar to those of the original experiment. For this purpose C.S. acted as percipient and Hansel as agent. We agreed upon a simple code before the meeting, thus: A and P memorized the sequence G P L Z E. In the odd-numbered runs of 50 these symbols were to be used by P on the 4th, 9th, 14th, 19th and 24th guesses of each half-run. In the even-numbered runs of 50 the same symbols were to be used on the 5th, 10th, 15th, 20th and 25th guesses. A was to arrange +1 displacement when the experimenters asked for the slow rate, and +2 when they asked for fast. (In the event, however, only one run was attempted at the fast rate, and for this we changed our minds and decided to keep to +1 for simplicity.)

Five runs of 50 were demonstrated, three PRN and two counters. After their completion C. S. discovered that he and Hansel had made an error in working out the method, in that the technique they were using was such as to give a -1 displacement, not +1. This error was, of course, of no very great significance since the method of card substitution can be used equally easily to obtain either displacement. (It occurred to us after the demonstration that in the first sitting with J. Al. as agent—the most successful agent in the Shackleton experiments—Shackleton also had scored -1 successes, but not +1. On all subsequent occasions he scored +. The parallel is perhaps suggestive.) The -1 scores for the five runs were: 7, 14, 11, 11, 16 (expectation 9.6). In the first run Hansel had considerable difficulty applying his technique (he scored 3 hits by cheating, and had the ill luck to score only 4 more by chance). Later he found the method becoming rapidly easier, and expressed confidence that he could quickly make himself proficient at very much more sophisticated techniques. C. S. as percipient found his task quite easy. If not watched by an observer he could have filled in the key guesses before the run, and in that case the method would have been extremely simple. In fact, however, he filled them in at the correct moment; he found that this required quite an appreciable degree of concentration, particularly at the fast rate. He once made an error, but it was, of course, easy to correct it at the end of the run. In C. S.'s opinion, though concentration was required, the task could not honestly be

described as difficult, and, like the agent's, it seemed to become rapidly easier with practice.

It will be noted that the above method of cheating was a little too simple to be safely used over a number of runs without detection. It would, however, have been easy for us to graduate to more complex techniques (varying the pre-arranged symbols and their positions in the run) after the amount of practice we obtained from these first five runs.

In all the experiments Mrs Goldney acted as E. A. and Dr West as Observer. After the formal experiments we tested a number of details in the experimental procedure, in particular the recording of the target sequence in the counters routine.

The following are my own conclusions from the demonstrations.

1. The percipient's task in the method of card substitution demanded only a small amount of skill and became easier with practice.
2. The agent's task demanded an appreciable amount of skill but became much easier with practice.
3. An observer sitting beside A would be very unlikely to see the *faces* of the five cards. The faces are not very easily distinguishable; the position of the box and of A almost inevitably means that the lighting on the faces is bad; and A is not likely to lift the cards high, particularly at the fast rate.
4. It follows that, in order to prevent card substitution by A, the observer would be required to keep an absolutely continuous watch on A's *movements* from the beginning of the run to the time when the card order is checked (this period includes the interval between the two half-runs). In order to perform his function, however, the observer should know which card A is supposed to be lifting, and he can only know this by glancing at the window in the screen at each trial. A continuous watch of A's movements is therefore almost impossible, and it seems at least conceivable that a skilled A could make his substitutions during the moments when the observer is looking at the window. (Note that only 5 substitutions at the most are required per run of 50, and that these can be performed at any time during the run.) Against this it must be granted that since the observer was somewhat behind A it was impossible for A to know exactly when he was not being observed. Card substitution was therefore dangerous, and it might reasonably be argued that if it had continued throughout a long series it is unlikely that it would not have been noticed at some time by the observer. On the other hand the movements involved in card substitution are not very suspicious, and it is not difficult to imagine an observer who had actually seen them failing to notice their significance and hence failing to report them.
5. Even apart from the observer's need to observe the window in the screen as well as A's movements, it seems to me rather unlikely that any observer would keep a continuous watch over A's movements unless he had been given very specific instructions to that effect. The present rehearsal of the Soal experiments brought out very clearly (to my mind) what an insignificant-seeming part of the whole procedure was the activity of the agent in lifting his cards under the

box. From an observer's point of view the psychological orientation of the whole experiment is towards *communication* between A and P. If he is suspicious he will watch sharply for some method of signalling. Unless he has explicitly thought of Hansel's method of card substitution (which clearly no one had) it will not occur to him that there is anything useful that A can do inside the box which might contribute to a spurious result.

6. In the counters experiments the observer sitting beside the agent had the additional task of recording the target sequence by observing the colour of the counter appearing at the window and translating it by an agreed code into one of the digits 1, 2, 3, 4, 5. I am perfectly satisfied that an observer so occupied could not give more than the most superficial attention to A at the slow rate of calling, and could not give any attention whatever to A at the fast rate, for the task is extremely demanding. Indeed, at the fast rate there would be no need for A to lift the cards at all; he could concentrate all his attention on card substitution and was completely free to perform any manipulation he desired in the box.
7. At the demonstration on 5 July we found that none of us was able to perform the observer's task of recording the target sequence reliably at the fast rate. On 7 July, however, Mr Parsons visited C. S. and demonstrated that, with practice, it was possible to perform this task even at the fastest rate of 26 seconds per 25 trials. The task took all the observer's attention, however, and it seemed clear that errors would be frequent.
8. My general conclusion from these demonstrations was that Hansel's method of cheating almost certainly could have been carried out under the conditions which probably existed in most of the original experiments.

This ends the extract.

To prevent any danger of misunderstanding, two very obvious limitations of this small enquiry should perhaps be made explicit.

a. We tested the card substitution technique under conditions which were intended to duplicate, in the main essentials, the *physical* conditions prevailing in *most* of the original experiments. No attempt was made to duplicate the psychological conditions, although these are obviously relevant to the cheating hypothesis. Nor was any attempt made to duplicate the conditions of *all* the sittings, which varied considerably. Thus the conclusions relate only to the main body of the experiments, and they leave open the question whether there may have been *on certain occasions* conditions which prevented card substitution.

b. The conclusion that card substitution was possible is only one among many pieces of evidence bearing on the question whether card substitution actually took place.

Finally, I should point out that this kind of test is easily repeatable, and anyone who is seriously interested in the problem would be well advised to carry out trials of his own rather than rely on the present report.

REFERENCES

- (1) Soal, S. G. and Goldney, K. M. 'Experiments in Precognitive Telepathy,' *Proceedings S.P.R.*, **47**, 1943, pp. 21-150.
- (2) Soal, S. G. and Bateman, F. *Modern Experiments in Telepathy*. Faber, London, 1954.
- (3) Hansel, C. E. M. *Proceedings S.P.R.*, **53**, 1960.

TELEVISION BROADCASTS

DURING February and March there were three consecutive programmes on the BBC-TV 'Lifeline' series dealing with matters of interest to the Society.

On 3 February, part of a recording of a sitting was broadcast in which the mental medium Mr Douglas Johnson gave information to an unknown sitter. After the recording it was revealed that the sitter was in fact the wife of the consultant psychiatrist Dr Stafford-Clark, who compères the 'Lifeline' series. The Society has acquired from the BBC a 16 mm. copy of the film of the complete sitting—a unique document which should be of the greatest interest to members.

On the same programme on 17 February Mr Johnson and Mrs Stafford-Clark were again interviewed, and the interpretation of the sitting was discussed by Mr Christopher Scott, Chan Canasta (the well-known television magician) and Prof. H. J. Eysenck (Professor of Psychology of the Institute of Psychiatry, London University).

The programme on 2 March was entitled Extrasensory Perception. During the broadcast four persons who had shown previous evidence of ESP were asked to guess a sequence of 25 random digits presented serially to the viewers. Results were in accordance with chance expectation. Dr Donald Michie, a geneticist of Edinburgh University, was interviewed on probability and statistics. After the result of the experiment had been accounted, Dr D. J. West was interviewed on its interpretation and on the general subject of ESP research.

The following note on the programmes is contributed by

Christopher Scott, who designed the experiment in the third of these broadcasts.

Any *brief* discussion of the Douglas Johnson television seance would almost inevitably be misleading and I shall not attempt it here. It is probably fair to say that the first programme tended to give the impression that the sitting was successful and could not be explained by 'normal' means, while the second tended to imply that it could.

Three matters of fact which were never made clear in the programme should be mentioned here, since psychical researchers will need to know them in order to assess the sitting.

1. It will be recalled that the medium said that the sitter's mother died of cancer of the stomach. The sitter states that her mother *believed* herself to be suffering from a stomach complaint at the time of her death, although in fact this was by no means the cause of her death, which was cancer of the lung.
2. Before the sitting took place, the BBC assured Mr Johnson that the film of it would not be used in the broadcast if unsuccessful. This fact is relevant to an assessment of the chance hypothesis. The 20-minute extract, however, which was broadcast on the first of the programmes contained almost the whole of the sitting. In the part of the sitting not broadcast only one statement (or possibly two) was made by the medium to the sitter. In the second programme, the first half of this 20-minute extract was broadcast again.
3. The sitter's watch, which the medium asked for and handled during the part of the sitting which was broadcast, had engraved on its back the words 'To Dorothy from David'.

In the programme on extrasensory perception four percipients in the studio guessed a random sequence of 25 digits, each indicating his guess on a dial in front of him. The target sequence was prepared by myself from a table of random numbers just before the experiment, and each target was shown to the viewer, inlaid in the top right-hand corner of the screen, just before each guess. Before the experiment the viewer was instructed to concentrate on the targets as they appeared; it is doubtful, however, if this instruction was given enough weight. One or more of the guesses were also visible to the viewer at any given moment.

The percipients were two young ladies (Pat and Shirley Clark) who were identical twins, Mrs Arthur Oram and Mr B. Downey. The twins claimed to have frequently experienced telepathic communication between one another and to have succeeded in card-

guessing experiments conducted by themselves. They were also tested some years ago by Mr Fisk, Dr Soal and Mrs Goldney, but with negative results. Their father had taught them a stage mind-reading act, but this of course did not depend on ESP. Mrs Oram also had had several experiences of apparent spontaneous ESP or precognition. Finally, Mr Downey had been the most successful percipient in a television experiment in 1956, designed by Dr Michie and broadcast on ITV, in which viewers were asked to send in their guesses (1). Mr Downey had scored at over twice the chance expectation, and although this result was not significant when allowance was made for its selection as the best of 1,367 entries, it was sufficiently suggestive to prompt further study. In follow-up work Mr Downey had obtained highly significant scores ($P < .00001$) with Mr Fisk as agent and he was still scoring high when the experiments were discontinued in August 1956.

Many radio and television experiments in ESP have of course been conducted in which the home audience plays the role of percipient, sending in a record of guesses for analysis. There seems to have been only one instance, however, of the reverse arrangement being used (a recent network broadcast in the United States) and on that occasion it appears that the data then collected were inadequate (2). It therefore seemed well worth trying such an experiment in the present programme.

There are two reasons for expecting success in such an experiment. Firstly, if ESP signals are additive then, clearly, the more agents the better, since those agents who can send the message will be reinforcing one another while the others will not. Secondly, if one imagines that outstanding agents exist but are rare, then the more agents we have the greater the probability of our including among them at least one who is outstanding. The present experiment was particularly promising because of the inclusion of Mr Downey as one of the percipients, for we were using, in Dr Michie's expression, a 'double dragnet'. In the 1956 television experiment the net had been cast among the viewing population and had duly caught one outstanding percipient. In the present programme we were casting the net out again in the other direction to catch at least one good agent. The combination of these two high-powered selection factors seemed promising.

In the event, however, the results were negative. Neither the group of percipients as a whole nor any one percipient scored significantly, either on direct hits or on -2 , -1 , $+1$ or $+2$ displacements. Divergence scoring (3), taking 0 as adjacent to both 1 and 9, was also tried for the zero displacement but with no better results.

It is difficult to see how any simple hypothesis of interference between the agents, or of cancellation of positive agents against negative, can explain these null results without introducing some rather artificial supporting assumptions, for the sample of viewers acting as agents could hardly be so well balanced as to allow perfect cancellation. It is certainly simpler to suppose that no ESP signals at all were getting through to the percipients. The reason for this is anyone's guess.

There was one defect in the experimental arrangements for which I must take the blame. We allowed a 5-second interval between targets, and just after the target appeared to the viewer a bell rang to inform the percipients. I had stressed to the percipients the danger of falling out of step, and as a result they all made their guesses within a second or so after the bell. This allowed very little time for the viewer to concentrate on the target. Often, no doubt, when he did begin to concentrate he found it was too late and he had to wait a further 4 seconds with nothing to do. While this mistake was unfortunate it is hardly likely to have accounted alone for the negative result, first because many thousands of viewers must surely have attended to the target with sufficient speed despite the difficulty, and secondly because many will have continued to concentrate even when it was too late to affect the result, and this should have produced a detectable retrocognitive effect for the next guess.

The producers, Hugh Burnett and Michael Johnston, and the studio staff often went to considerable inconvenience to meet the unusual technical demands of our subject, and we owe them a considerable debt of thanks. Thanks are also due to the four percipients, to the four recorders who worked under the supervision of Mr Oram, and to Mr Parsons who acted as an observer in the control room.

REFERENCES

- (1) Michie, D. and West, D. J. 'A mass ESP test using television.' *Journ. S.P.R.*, **39**, 1957, pp. 113-133.
- (2) Nash, C. B. 'The Chesebrough-Pond's ESP television contest.' *Journ. American S.P.R.*, **53**, 1959, pp. 137-138.
- (3) Fisk, G. W. and Mitchell, A. M. J. 'ESP experimens with clock cards.' *Journ. S.P.R.*, **37**, 1953, pp. 185-197.

REVIEWS

THE BOY AND THE BROTHERS. By Swami Omananda Puri.
Gollancz Ltd, London, 1959. 304 pp. 21s.

This is a remarkable record of a case ostensibly of multiple possession. The subject (here named 'the Boy') was an uneducated London artisan. The author is an Irishwoman who had been a musician of considerable distinction. She writes under the name she took when she received the *sannyasa* initiation in India much later. She records that she is the first woman who has received that initiation and become a Swami for about 2,000 years.

She first met the Boy when she was trying to find a house in East London to use as a centre for curative music. He came to live in the house she bought and the centre developed in a way quite different from the one she had originally intended. The Boy developed trances in which he appeared to be possessed by intelligences who called themselves 'Brothers' and used the Boy as a medium for healing and teaching, first in London and afterwards in India where the Boy died in 1956.

This book gives an account of the history and characteristics of the Boy's trances and some brief specimens of the Brothers' teachings. It is hoped that a fuller account of the teachings will appear in a later book. The specimens of the teachings given here make it clear that they are not of the trivial character often found in what purport to be messages from another world. 'Live from moment to moment. In such a living, there is no room for fear, which belongs with memory and anticipation when these are coupled with effortful attachment' (p. 212). 'Ignorance and learning keep wisdom in bondage' (p. 205). 'There is nothing in your world, either alive or dead, that is worth being agitated about, except the alleviation of suffering' (p. 207).

These are not trivial but the evidence as given in this book is insufficient to enable one to decide to what extent the teaching is original, in the sense that it could not be paralleled in existing religious writings. It seems clear from the accounts given in this book that the Brothers often spoke in a way appropriate to the particular problems of the individuals they were interviewing. Whether their teachings also contain a message appropriate to the problems of our civilisation in general must be judged when the fuller body of the teachings is published. The specimens given in this book are sufficiently striking to arouse an appetite for a fuller version of the teachings.

The question of particular psychical research interest is that of

the source of the Brothers' teachings. The author believes that they were given by discarnate personalities of great wisdom and power. The sceptical reader may think of other possibilities. That they were products of the mind of the Boy himself is very improbable. The teachings were recognised by Indians as belonging to the tradition of the Upanishads and the Bhagavadgita. Even a highly educated European could not hope to produce in a cultured Indian audience the impression that his thoughts were upanishadic, and it would be plainly impossible for the uneducated Boy. The Swami herself seems a more likely source. It is true that she reports that she has read little, but she was certainly read to when she was a child, and the New Testament and the Gita were among her devotional books at a relatively early age. She does, in fact, discuss the theory that she was the source of the Brothers' manifestations and argues strongly against it (p. 139).

That the Brothers were merely alternating personalities of the Boy himself subconsciously informed by the Swami is not, of course, the only alternative to the theory that they were discarnate personalities occasionally possessing the Boy. It must also be considered that they may have been composite personalities with elements contributed by the Swami and by others consulting the Brothers, with the further possibility of more or less of these composite personalities being contributed from extra-human sources. It must be admitted that if the accuracy of the Swami's account is accepted, it is difficult to make it support any other view of the Brothers than the one she accepts, that they were discarnate personalities controlling the Boy when he was in trance. They would seem to have had considerable but not unlimited paranormal powers. They seem to have foreseen the danger of war in 1939 (which was however more generally foreseen than the author implies) but not to have foreseen the grave dangers the Boy would be in from the Hindu-Moslem massacres which followed Indian independence. They seem to have healed some ills and to have been unable to heal others. If paranormal powers are real it is to be expected that they will have limitations; it is hard to know what these limitations will be. It may be that the Brothers' teachings have the wisdom of Eternity but it is also possible that their appreciation of the problems of our own times may be at fault. We must wait for the fuller account of the Teachings to judge how far they contain a message for the present day.

Perhaps the sceptical may wonder whether the whole thing is a fabrication. Hoaxes in the occult field are not unknown. I can, however, give a personal assurance that this is no hoax. I have met the author and I have seen letters about the Brothers both from

Professor Daniel Jones and from Indians of academic standing. Whatever may be its explanation, something odd has happened. It is to be regretted that it is now in the past and that only the written records remain for our study.

R. H. THOULESS

FORTY YEARS A MEDIUM. By Estelle Roberts. Herbert Jenkins. London, 1959. 199 pp. 18s.

Ever since George Redman, the Boston medium, printed his autobiography in 1859, a few mediums have followed his example and there is not one of these books which is not revealing to the discerning student, especially if he is able to compare the stories told by the author with similar tales from other sources.

The latest of this series of autobiographies is that by the famous English medium known as Mrs Estelle Roberts. The book appeared in a blaze of publicity, being serialized in a number of journals both here and abroad. Mrs Roberts is called the world's most amazing woman and the greatest medium of all times, who for sixty years has talked to the departed and has baffled the best brains in the world.

In the course of her book Mrs Roberts tells the story of her life and gives example after example of her astonishing powers. As a platform clairvoyant she has been known in London and the provinces for years, but in addition to exhibiting her mental phenomena she has demonstrated the direct voice, materializations and apparitions. Apart from these phenomena, Mrs Roberts, or rather her guide Red Cloud, has been able on occasion to exhibit the most surprising control over wind and flame, and as a healer, with Red Cloud acting as spirit surgeon, she was able to remove a splinter of bone from a patient's back, the physician present searching in vain for a puncture through which the bone could have passed.

Mrs Roberts has demonstrated her clairvoyance to the police and both in the serialization and in the book itself the story is told of what happened when she told the police where to find the body of the murdered child Mona Tinsley in 1937. The child was missing in January of that year and Mrs Roberts says that in June the police found the body at exactly the spot she had told them they would do so. This tale that Mrs Roberts indicated the place where the police later were to find the body was in the serialization but not in the book. The fact is that it was not the police who found the body and there is no evidence that the exact spot was ever indicated by Mrs Roberts at any time. What we are not told is that during the

course of the case a good many clairvoyants had written to the police indicating where the body was to be found; and the best of them was not Mrs Roberts but a Bristol astrologer, who stated that the body would not be found until the first week of June and that it was close to an open meadow with tall trees along the bank and some thirty miles north-west of Newark. Actually it was found in the River Idle some twenty-seven miles north-north-west of Newark on June 6.

Although Mrs Roberts has been demonstrating her versatile mediumship for nearly forty years I have no record of a single sitting for the purpose of investigation by independent and trained psychical researchers. It is Mr Maurice Barbanell who is privileged to stroke Red Cloud's long silky hair, and it is Mrs Treloar who cuts the locks from the head of Archael, the guide, and it is into her hands that the apports fall. Unlike the majority of D. D. Home's seances, where darkness was not permitted, Mrs Roberts' physical circle was held in complete darkness, since Red Cloud objected to even the slightest pin-prick of light, for this interfered with the 'psychic rods' which are supposed to wave the trumpets about. Thus the reputation which Mrs Roberts has built up rests almost entirely on articles contributed by journalists and untrained sitters with whom successes have been achieved. There is absolutely nothing of the slightest scientific value. 'The greatest medium of all time' will go down in history as one who has never encouraged any inquiry of the kind welcomed by Mrs Piper, Mrs Leonard, D. D. Home or Eusapia Palladino. No independent inquiry has ever been undertaken which, even after a short period, would have established the true character of the direct voices, the materializations and the apports. Why this is so must be left to the judgment of the reader of Mrs Roberts' fascinating book. It should be read by all students of the subject since it is only through books like this that he will be able to understand how the right kind of publicity, coupled with successes with prominent people, can persuade journalists to compare her, as one did, to Leonardo da Vinci, for just as he showed his genius in both science and art so does Estelle Roberts display her powers in both physical and mental mediumship.

E. J. DINGWALL

ARTICLES AND CORRESPONDENCE ON E.S.P. IN ANIMALS. *In Country Life.*

25 Dec. 1958. An article entitled 'The Uncanny Place' by H. T. gave an account of incidents at a ruin 600 yards from the A.697

road between Morpeth and Wooler, Northumberland. His mare refused to cross a bridge in the vicinity leading nearer to the site. (There is no evidence as to the animal's disinclination to cross bridges in general, or as to the state of the bridge. A horse would naturally refuse to cross an obviously unsafe structure. But would a horse refusing to cross a bridge break out into a sweat and show the signs of distress that this one showed?) At a later date he took two dogs there to let them hunt. There were no rabbits, no birds. Everything was uncannily quiet. It was damp and misty. The dogs just sat down miserably. When they left the place, the dogs recovered their spirits and the birds and rabbits frolicked.

In subsequent numbers of *Country Life* there was correspondence and contributors related some interesting experiences and suggestions as to the causes of the uncanniness of the place.

22 Jan. 1959. A correspondent thinks the 'ultrasonic' call of the blackcock (which is found locally) may have accounted for the fear of the animals. (But why should it have alarmed them so unnaturally?)

29 Jan., 5 & 12 Feb. 1959. There are three letters telling, but with no great precision, of a dog's fear of John of Gaunt's residence near Leicester, a pump, and a certain lane and room.

26 Feb. 1959. There is a fairly circumstantial letter from J. P. B. who is evidently observant of things that matter. He relates an incident that happened in India, when his dog suddenly stopped dead, on a familiar road, staring at 'something' between him and his master, through which his master must have just walked. He avers that the 'thing' the dog was afraid of was nothing it had heard or smelled; it had definitely seen something.

There is also a rather vague account of four horses that did not like to go near a gate, and a puppy that would never budge beyond a certain point when he reached a churchyard.

5 Mar. 1959. Lady L. tells how a favourite dog *always* shivered in apparent terror at a place where a witch was alleged to have been murdered.

26 Mar. 1959. Lady O. says that Moor Park (where the above incidents took place) belonged to her husband's family and she confirms that a witch was supposed to have lived there in a cave in the 14th or 15th century. C. Q. writes to say that there is no historical mention of 'Mother Ludlan' (the witch) being murdered, but some half-wit is supposed to have starved himself to death in a neighbouring cave.

9 April 1959. R. C. B. G. tells a fascinating story, unfortunately at second/third hand. Some friends were driving a pony and cart over a highland pass when suddenly the pony became very unhappy, started shivering, and finally lay down. After some time, a 'highland gentleman' appeared from nowhere, complete with ghillies and dogs, and said 'That way lies your road to Fort Augustus.' Several persons heard this and saw the party. As soon as the 'highland gentleman' had appeared and said his piece, the pony got up quite calmly and returned to normal. The deer in the vicinity had ignored the strangers who later were 'identified' as the well-known ghost of Corryarrick and his retinue.

23 April 1959. M. M. has a story about an Eden Cottage, near Edinburgh. His dog used to stiffen at times, the hair would rise on its back, and it would seem frightened. One night his father woke in alarm, sensing a presence in the room; as he awoke the dog burst into a howl.

Subsequent tenants bred Bedlington's and the effect on these dogs seems to have been the same and the tenants had to leave.

30 April 1959. A correspondent who apparently is prepared to look for natural explanations points out that dogs can misinterpret sights. His dog used to mistake an old wheelbarrow for another dog, and growl. But he goes on to tell when he was living in a 17th century house, this dog used occasionally to stiffen, become very intent, and walk round the house, from one room to another, and up and down stairs, in slow motion, as if following something—something that was evidently at the dog's eye level.

There seems to be some evidence for animals seeing, hearing, or sensing the paranormal. There are, after all, plenty of noises in an ordinary house quite inexplicable to the dog (creaking of wood, clanking of boilers, gurgling of radiators) but dogs don't spend the day getting up to investigate in the tense manner described. What sort of sight or sound would then induce this sort of behaviour? Assuming the good faith of even a small percentage of the contributors it seems reasonable to suggest that it might be a sight or sound beyond the normal. *Homo sapiens* undoubtedly sometimes sees apparitions; why not *canis domesticus*?

M. R. BARRINGTON

THE HAUNTED MIND: A Psychoanalyst Looks at the Supernormal.
By Nandor Fodor. Garrett Publications. New York, 1959.
\$5.00.

This book contains a collection of paranormal cases investigated 25 years ago, when the author was the Research Officer of a

Spiritualistic Society in London. Events changed his course and he became a successful psychoanalyst in New York. So besides his notes and comments on the cases when he investigated them he now brings to bear on them his knowledge and experience as an analyst. This shows how much his wider experience with all kinds of maladjusted people, as patients, lights up the darker aspects of psychical research.

He links up the 'haunted mind' with the haunted house. The cases are very varied in content and published here for the first time except for a brief mention of the Thornton Heath case. Those who like ghost stories would find this an entertaining book but for psychical researchers there are many valuable points and suggestions. The idea that a ghost in an ancient haunted house remains there, in the unseen world, complete in its setting and date in history, in a similar way that repressed emotions and fears remain in the unconscious mind of the individual may be a new conception, especially as it seems both can be dissipated in the same kind of way, i.e. by linking the past with the present, bringing the detached, repressed ideas or fears into the conscious mind of the individual and integrating them with the rest of the psyche.

The case of the Ash Manor Ghost illustrates this. Ash Manor House in Sussex was originally built in the time of Edward the Confessor. In 1934 it was occupied by the Keel family. The disturbances started by a series of violent bangs. Eventually both Mr and Mrs Keel, independently, saw an apparition of 'a little oldish man, dressed in a green smock, very muddy breeches and gaiters, a slouch hat on his head and a handkerchief round his neck. Eventually Dr Fodor was called in to see if he could rid them of the disturbances. Mrs Eileen Garrett consented to give a sitting in the room. After returning Uvani's (Mrs Garrett's 'control') greeting Dr Fodor asked: 'Is it possible to do something for the Keels and the ghost?' Uvani replied: 'You will not mind if I say there is unhappiness in the house and if there is an impression of something coming back it is because you make for that spirit a garden of memory in which it can live and revive its sufferings.' (Dr Fodor discovered that the relationships between the husband and wife, and also wife and daughter, were very strained.) Uvani said he would let the spirit take possession of Mrs Garrett and speak for himself. The ghost, through Mrs Garrett, talked a great deal, some sense, some nonsense, but there was no disguising his state of unhappiness; he was thirsting for revenge and so on. Enough to give Dr Fodor the clue. He knew then where to look for the cause, and though he was not then a

psychoanalyst, he found it, and he shows in his account how he was able successfully to exorcize the unhappy ghost.

In biology we seem to have the same sort of pattern. The life of the past is buried in the strata of the rocks until the geologist digs it up and revivifies it. Perhaps the same applies to our psychic past.

ZOE RICHMOND

CORRESPONDENCE

G. Spencer Brown and Probability

SIR,—In the course of my article entitled 'G. Spencer Brown and Probability' in the S.P.R. *Journal* for June 1958, I made two statements to which Mr Spencer Brown has taken exception. I have reconsidered these statements carefully and would be grateful if you would allow me to publish the following clarification:

1. On page 233 of my article I described in my own words an argument which I said Mr Brown used in 1953 in *Nature*, and I went on to say, '... eventually Brown denied that he had used the argument at all'. I referred to the Third London Symposium on Information Theory, at which Dr I. J. Good attempted to describe Mr Brown's argument in question and Mr Brown replied simply, 'I have never used the argument he (Good) has imputed to me'. I described this as a denial by Mr Brown of having used an argument that he *had* used, because in my view Dr Good's version was substantially a correct account of Mr Brown's argument. However, this was only my opinion and if I failed to make this clear I offer my sincere apologies.

2. On page 233 I also wrote: 'Brown responds readily to criticism with a modification of his position, but generally only when confronted with the critic; he is liable to relapse when the pressure is removed. An excellent view of this process at work can be obtained by comparing his address to the information theory symposium, including his reactions to the criticisms of Mr Mayne, with the revised version which constitutes Chapter XIV of the present book.'

The facts are these:

a. Extract from Mr Brown's address to the Symposium

'One of the more constant observations (of psychical research workers) has been that patterns which have built up a high degree of significance in long series of randomizing or matching data invariably disappear when the series is continued after the patterns have been noticed'.

b. Extract from Mr Brown's reply to Mr Mayne at the Symposium

'He (Mayne) says there is no factual basis whatever for my assertion that the highly significant matching patterns in psychical research 'invariably disappear when the series is continued after the patterns have been noticed'. My use of the term 'invariably' is, perhaps, open to comment. It is quite possible, as Mr Mayne says, to find counter examples; but these counter examples are neither typical of the work, nor have they been obtained by any reputable investigators, other than, perhaps, Rhine and Soal'.

c. Mr Brown's book (Probability and Scientific Inference, p. 105)

The passage (a) above is repeated in the book, with the omission of the word 'invariably'.

The justice of my description of this as a modification of position followed by a relapse, depends upon the truth of the two following propositions:

- (i) That the last sentence of (b) above constitutes a substantial qualification of (a).
- (ii) That the omission, in (c) of the word 'invariably' does not, in the context, substantially modify the meanings of (a).

It is no doubt best that your readers judge this issue for themselves.

CHRISTOPHER SCOTT

The Ultrasonic Whistle

SIR,—Christopher Scott and Mrs Goldney are to be congratulated on having experimented on the possibility of the use of the ultrasonic whistle and not merely speculated about it. They have clarified many points.

There is, however, one question in their article which requires comment. The authors ask why no one has mentioned the ultrasonic whistle as a means of counterfeiting telepathy. Surely one answer is because the situation in which such a possibility could arise has been very rare in experimental work. There have been very few cases in which the agent has been one of the experimental subjects. Generally the agent has been the experimenter or one of the experimental group. What these experiments do show is that in experiments under telepathic conditions, we must rely on the good faith of the experimenter, and that the use of a multitude of 'witnesses' does not guarantee the genuineness of results. Particularly they serve to warn us against the common superstition of the special value of the conjurer as a witness.

In much of the discussion that has taken place, there seems to be a failure to distinguish two questions: (i) Did the Jones boys use

an ultrasonic whistle? and (ii) Were the conditions of the experiment such as to preclude the possibility that this method was used? The second question is, I think, the important one. If the answer to this were 'No', it would be of little profit to speculate about the answer to the first question.

R. H. THOULESS

SIR,—In an article *The Jones Boys and the Ultrasonic Whistle* in the March 1960 *Journal*, by Christopher Scott and K. M. Goldney, the authors pose the following question on p. 260: If a new pair of apparently successful 'mind readers' present themselves, what experimental precautions would be necessary next time to rule out ultrasonic signalling?

It should be possible to exclude the use of an ultrasonic whistle by means of a thorough search of the agent, but such a procedure has certain drawbacks. If the agent is innocent of any trickery the search will hardly be conducive to the pleasant atmosphere favouring ESP results, and the sceptic, who reads about the experiments, may not feel convinced that the search was sufficiently thorough. If, then, it may not be possible to rule out ultrasonic signalling, it is possible to discover such signalling by the use of a so-called condenser microphone for ultrasonic measurements, connected with a measuring amplifier which has a dial and pointer. Such an instrument will register any frequency up to, say, 40,000 Hertz.

AAGE SLOMANN

Poltergeists and Underground Water

SIR,—Mr G. W. Lambert has given (*Journal*, 38, pp. 49, 201; 40, p. 108) a very plausible theory to explain many poltergeist phenomena by the presence of unusual movements of underground water. So far, however, no statistical significance test has been applied to the data, and I feel something could be done along these lines. As an example, consider the Scottish haunts study, *Journal*, 40, p. 108. Of the 87 years 1868–1954, 41 were classified as wet, 46 as dry years, whilst of the 25 dated poltergeist records, 14 occurred in wet years, 11 in dry. Now the probability of obtaining as extreme a result as 14 'successes' in 25 independent events, each with a hypothetical probability of occurrence $p=41/87$, is roughly $P=1/2$. This is not very convincing from a statistical point of view. As further evidence of his theory, Lambert states that 5 of the 11 'failures' followed runs of two or more wet years. Unfortunately, the original data were not presented, but the proportion of such runs in the period could be compared with 5/11. As

Lambert admits, the small number of cases makes it difficult to achieve significant results.

If the theory is correct and a large proportion of phenomena can be so explained, then one could expect to obtain significant results, even from the number of cases already available, provided a reasonably sensitive measure of underground water activity could be found. I immediately think of comparing the week's rain or tidal heights, previous to an occurrence of poltergeist activity, with the long term average, and testing the proportion of occasions on which the average is exceeded (a distribution-free test) or using a 't' test for the actual excess.

Finally, a warning. Whilst some sort of significance test is called for, one difficulty seems to warrant consideration, and that is the question of independence of the observations. If newspaper accounts, and the like, of one haunting trigger off a whole series of reports at the same time and in the same locality, a significance test treating these as independent could be quite misleading, and so too could be visual inferences drawn from location maps.

G. A. WATTERSON

Comment by Mr G. W. Lambert on the foregoing letter

I agree with Mr Watterson that the 'underground water' theory needs to be put to a statistical significance test, and I believe that with perseverance enough data can be collected to enable that to be done. Scotland, though interesting from the geological point of view in connection with this problem, is an unsuitable area in which to apply the rainfall test. It is too large, and the rainfall averages for the whole country iron out local extremes, which are probably important factors in the reckoning. Further, the areas in eastern Scotland, which on geological grounds are most likely (according to the theory) to produce phenomena, are those on which rainfall is lowest. I had thought by now to have come across some data which, if the theory is unsound, would conclusively disprove it. But instead, I keep finding slight, but not significant, results giving it just enough support to make further tests worth while.

For a satisfactory application of the rainfall test one requires a relatively large number of cases in a small area, covering a long period of years. I am now working on the London collection of about 100 cases in the Society's records, covering almost a century, my object being to correlate them (*a*) with the underground geography of London, and (*b*) with contemporary rainfall figures. In the collection of data there are many pitfalls, especially in the

matter of dating occurrences. As most of the cases in the collection were not published, or, if published, were narrated without disclosing actual names of persons and addresses, the sample is not likely to be seriously affected by the sort of 'triggering off' effect to which Mr Watterson refers in his last paragraph. In the few cases I have come across in which two or more incidents have occurred in the same street, or immediate neighbourhood, they have been separated by such long periods of years (10 to 30 years) that 'imitation' seems a very unlikely explanation of the local conjunction of events.

If Mr Watterson will await the result of this further study, I look forward to receiving his help in the evaluation of the data presented.

A Fake Apparition

SIR,—The S.P.R. *Journal* for September 1959 carries a fascinating account by Mr A. D. Cornell of an experiment in the observation of a fake apparition. The 'ghost', wrapped in a white sheet, walked in a field for $4\frac{1}{2}$ minutes on six evenings at twilight. Although clearly visible to the official observers who knew what to expect, it was apparently unnoticed by every one of about 80 passers-by.

I would like to offer a possible explanation to account for the failure of so many people to see what they *could* have seen. Though disappointingly humdrum, this explanation is, I think, in line with modern psychological knowledge.

When we look at an object in good light we see it at once. But in poor light our attention must rest on the object for some time before we have collected enough visual information to convince us of what we see. Once we have this conviction we appear to see the object directly and contemporaneously, and we forget that the percept took a second or two to develop. Thus the observers, who knew approximately where to look and what to look for, saw the ghost within a second or two, and once seen it became, to them, 'clearly visible'. The passers-by, on the other hand, were not interested in the field. They may have given it a glance from time to time, but they would surely not have given it the concentrated and relatively prolonged attention which was necessary to reveal the ghost. Why should they?

The ability of a visual object to attract attention, and its clarity once it is attended to, are much the same thing in a good light. But in poor light this relationship no longer holds. Unfortunately our almost instinctive ideas about visual perception are based on what happens in good light.

If this explanation is correct it is the observers who were the victims of a sort of optical illusion—not, of course, in seeing the ghost, but in thinking that because they could see it clearly it was clearly visible to others.

CHRISTOPHER SCOTT

SIR,—*An Experiment in Apparitional Observation* (A. D. Cornell, *Journal*, September 1959); since (a) undergraduates are apt to get up to anything—even in the pre-examination month of May—and that therefore (b) the Cambridge attitude is to pretend to ignore anything unusual, would it not be wisest to discount Mr Cornell's point (2) on p. 123 ['some people saw it but did not consider it abnormal'] and repeat the experiment in a non-University town?

MICHAEL C. PERRY

The Jones Boys' Telepathy Experiments

SIR,—On the morning of August 28th 1956, in the S.P.R. Library at Tavistock Square, the boys produced some very high card calling scores in the presence of the late Mr Jack Salvin of the Magic Circle. I was called in to witness them. Dr Soal was not present and Mr H. T. Bowden was in charge.

After witnessing two runs (in which scores of 21 out of 25 and 20 out of 25 were produced) I persuaded the boys to try the clock face cards as targets so as to have twelve choices in the place of the usual five. The new cards were demonstrated to the boys most carefully, and they raised no objection. They completed three runs of clock cards, that is a total of thirty-six guesses, with a chance expectation of three correct calls. They obtained only one. Immediately after they reverted to their usual five card targets and at once regained a high scoring rate, scoring 18 and 19 in the following two runs.

On the hypothesis that the boys employed a simple code an increase in the number of target choices might be expected to interfere. The fact that they did fail under these conditions seems to me worth while placing on record. It has not been mentioned in Soal and Bowden's book *The Mind Readers*. The session in question is described on pages 179–81.

D. J. WEST

The Palm Sunday Case

SIR,—I have read the Palm Sunday Case and am impaled by the horns of the dilemma:

- (1) Either the scripts were inspired by discarnate intelligences or
- (2) they were the result of telepathy and clairvoyance of such scope,

intricacy and purpose that they make the feeble card games experiments of Rhine, Soal and the others look like very, very small potatoes indeed.

PETER MADDELEY

EXCERPTA

Further sayings of 'The Brothers'. See review in this issue, p. 312.

THERE is nothing 'abnormal' in the world—there is only the lack of understanding of the normal.

Get out of time! Let time pass about you.

We do not leave our bodies—our bodies leave us.

Remember it is with the ageing of the body that the soul is apt to get more truly youthful in its outlook and aspiration.

There are no beginnings and no endings.

Do not reach after the moon when you have the stars within your grasp.

There is much to do in old age which you cannot do when you are young. Do not try to be middle-aged or young. Accept the limitations of old age and work with Nature. Prepare for death. Get half the dying done before this body has to go.

From 'Experiments on Telepathy in Children' an article by Cyril Burt in 'The British Journal of Statistical Psychology', May, 1959.

THE aim of physical science is to deduce as wide a variety of facts as is possible from the smallest number of the simplest postulates. Its postulates therefore are laws of a theoretical model, not laws of nature. Mr Hansel would be fully justified if he treated his own 'laws' as stating what would be impossible in the particular theoretical model he proposes to try out first of all; but he certainly is not justified as treating them as infallible axioms incapable of modification or revision. Since 'the physical processes in the nervous system' to which he refers are now known to depend in part on processes of a sub-atomic order, we can no longer insist that the 'mental processes' associated with them must necessarily conform with a version of the 'laws of mechanics' which no longer hold good within the atom. Accordingly, we are entirely free to construct hypotheses that violate every one of Mr Hansel's principles,

if the facts require it. And indeed, . . . most of them are already violated by accepted hypotheses in other fields.

From a Travel Agent's advertisement in 'Harper's Magazine', New York, October, 1959.

TALK to the people of Scrapfaggot Green in Essex. They will tell you about a witch's sprite that was set loose by a U.S. bulldozer. It tolled the church bell and caused chickens to drown themselves in water butts.

Meet the Londoners who run the Drury Lane Theatre. Their peripatetic ghost appeared to a packed house—and it even wobbled to see *Oklahoma*. Don't expect all British ghosts to squeak and gibber. Some are really quite charming. The present tenants of Lady Hamilton's old house in London have only one complaint. The beautiful phantom leaves the doors unlocked. And then there are those supernatural dogs who answer to unlikely names, like Shriker, Trash and Shuck.

Long nights are the best for ghost-hunters. So why not visit Britain in the Fall or early Spring? In these lovely seasons you get the best of *both* worlds. A round-trip transatlantic ticket can cost under \$400.

NOTICES

The Society's new President

PROFESSOR HENRY HABBERLEY PRICE, F.B.A., M.A., B.Sc. has been elected President for the year 1960–61, in succession to Professor C. D. Broad. Professor Price, who was Wykeham Professor of Logic at Oxford University 1935–59, is at present delivering the Gifford Lectures at Aberdeen University. He was President of the Society in 1939–41 and has contributed important papers to *Journal* and *Proceedings*, notably his paper on 'Survival and the Idea of "Another World"'. (*Proceedings*, 50, January 1953.)

Instructional Groups

It has been suggested that members should have more opportunities for receiving instruction on various aspects of psychical research. Any member who is interested in receiving oral instruction in a particular field (e.g. Experimental ESP or Mediumship) should write to the Secretary-General stating the subject in which he is interested.

The Fourth McDougall Award

THE staff members of the Parapsychology Laboratory of Duke University have voted to present the fourth McDougall Award for Distinguished Work in Parapsychology to Dr S. G. Soal and Mr H. T. Bowden for their recently published book, *The Mind Readers*. The research grant of \$1000 which accompanies the award has been placed at the disposal of Dr Soal, for use in continued work 'as circumstances dictate'.

The Award, established in 1957, was named in honour of the late Professor William McDougall, F.R.S., Chairman of the Department of Psychology at Duke and sponsor of the Parapsychology Laboratory. He was President of our Society in 1920-1. Action is taken annually to determine the most outstanding research contribution published during the preceding year by workers not on the Laboratory staff. The basis of the award is international in its extent. Previous awards have been: Superintendent J. G. van Busschbach of Amsterdam (1956), G. W. Fisk and Dr D. J. West (1957) and Engineer Haakon Forwald of Sweden (1958).

The Indian Journal of Parapsychology

SUBSCRIPTIONS for the *Indian Journal of Parapsychology*, the quarterly organ of the Seth Sohan Lal Memorial Institute of Parapsychology, of Sri Ganganagar, (Raj.), India, may now be paid (17s. per annum) to their agents Wm. Dawson & Sons, Ltd., Cannon House, Macklin Street, London, W.C.2. This Indian periodical is of great value to all those interested in the reactions of Indian thought and philosophy to the problems of psychical research. It is edited by Professor H. N. Banerjee in collaboration with professors of several Indian universities. The publication has been established 'in the belief that parapsychological study, the alleged paranormal capacities of the human mind, cannot advance without benefit of knowledge offered by other scientific disciplines, while the scientific community cannot afford to ignore its challenge.'

ADDITIONS TO THE LIBRARY

South Seas Magic by Ronald Rose. Robert Hale, London, 1959. Price 18s.

Fourth Dimensional Man by Sam Solaryk. Pageant Press Inc., New York, 1959. Price 10s.

The Phenomenon of Man by Pierre Teilhard de Chardin. Collins, London, 1959. Price 25s.

The Haunted Mind by Nandor Fodor. Garrett Publications, New York, 1959. Price \$5.00.

The Boy and the Brothers by Swami Omananda Puri. Gollancz, London, 1959. Price 21s.

Please note following corrections to previous lists:

An Experiment in Mindfulness by Admiral Shattock. Rider, London, 1959. Price 12s. 6d.

Extra-sensory Perception, Witchcraft, Spiritualism & Insanity by Alastair W. MacLellan. The C. W. Daniel Company Ltd., Ashington, Rochford, Essex, 1958. Price 7s. 6d.

PRINTED IN GREAT BRITAIN
BY ROBERT MACLEHOSE AND CO. LTD
THE UNIVERSITY PRESS, GLASGOW

